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# THE PSYCHOLOGICAL BULLETIN

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## THE NEURO-PHYSIOLOGICAL CORRELATES OF LEARNING AND INTELLIGENCE \*

BY H. F. HARLOW

*University of Wisconsin*

### I. PSYCHOLOGICAL THEORIES OF LEARNING AND INTELLIGENCE

#### *A. Historical Summary and Introduction*

The manner in which learning and intelligence are conceived from the psychological standpoint must invariably influence the formulation and interpretation of researches on their neuro-physiological correlates. The importance of this fact may be best illustrated by considering briefly outstanding examples of the manner in which psychological theories have influenced the work of earlier investigators.

The observations of Gall (23)<sup>1</sup> show remarkable insight into the nature of mental disorders caused by circumscribed cerebral injuries. Yet a false psychological theory, based on the assumption of the existence of mental faculties, led him to formulate a theory of cortical localization so erroneous as to be devoid of scientific value. The theories of learning inherent in the associationist doctrine of psychology have likewise been a deterrent rather than an aid to neurologists in the interpretation of their data. It was associationism that led the nineteenth century "diagram makers" to try to explain aphasia in terms of the sensory images, and to try to localize these images in discrete visual, auditory, and kinesthetic centers. The

\* The author wishes to acknowledge the assistance of Mr. Paul Settlage for many helpful suggestions concerning the form and content of this article.

<sup>1</sup> A critical discussion of some of Gall's most striking observations are given by Head (34, Vol. 1, pp. 9-11).

observations made by many of these "diagram makers"<sup>2</sup> were extremely acute; their errors arose from attempts to shape empirical facts to conform with preconceived hypotheses.

One of the most original and ambitious of modern attempts to avoid the errors of the past and to classify aphasia by appealing to the nature of psychological disorders was made by Head (34). Head, however, devised a four-fold classification of mental functions which is at variance with prevailing psychological and neurological classifications, and there is every reason to believe that Head's unique psychological position has seriously limited the acceptance, and has even been detrimental to an understanding, of his monumental work.<sup>3</sup>

That psychological theories are always of importance in shaping and determining the direction of research on the neural correlates of the higher mental processes is amply substantiated by comparing and contrasting the views held by such eminent modern investigators as Lashley (56), Goldstein (27), Head (34), Piéron (88), Weisenburg (113), Henschen (35), and Kleist (50).

Thus Henschen (35) makes the following denunciation of Head (a denunciation which Henschen would apply to all "dynamic" concepts of neural functions): "With a priori conceptions he [Head] has made no analyses of aphasia material or of forms of aphasia, and his theory lacks any scientific confirmation and is therefore outside any scientific discussion. The influence of Head's doctrines is one of complete confusion." In somewhat less vigorous terms Head (34) writes of Henschen's work as follows: "In spite of these difficulties in fitting the clinical manifestations into the rubric of anatomical terms, Henschen propounds the most extreme views of cerebral localization. He not only upholds the classical separation of the varieties of aphasia into 'word-blindness,' 'word-deafness,' and the motor forms, but constructs a diagram showing centers for speaking in the third frontal, for writing in the second frontal and for reading in the angular gyrus. . . . Such extreme mechanical conceptions have been adopted by few observers."

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<sup>2</sup> The term "diagram makers" was coined by Henry Head, who gives an excellent, comprehensive review of the history of aphasia up to the time of his own work. The early data is also briefly summarized by Weisenburg (113), who has brought the available data up to date and has subjected it to a careful, critical analysis.

<sup>3</sup> Weisenburg (113, pp. 49-50) states that, ". . . the subsequent literature has shown a rather widespread understanding and appreciation of Head's theory of aphasia as a disorder of symbolic formulation and expression. . . . Experience has shown that there are forms of disorder which do not correspond to any one of the four types, others which must be related to more than one type, and still others which manifest only part of the disturbances Head described as characteristic of the type."



### B. Existing Theories of Learning and Intelligence

Modern psychologists are in general agreement regarding the errors made in the past in the formulation of theories of learning and intelligence. "Faculty psychology" is now a medieval superstition and "association psychology," if credited at all, is so disguised as to be hardly recognizable as the association psychology of the past century.

There is, however, marked disagreement in modern formulations of the fundamental psychological nature of learning and intelligence. As Tolman (109) has pointed out, there exist at the present time 2 opposing positions: (1) a position based upon "reflex arc theories," and (2) a position based upon "field-force" theories of learning. Tolman further subdivides the first group into (1) conditioned reflex theories of learning and (2) trial-and-error theories of learning. Followers of the "reflex arc theories" who have presented a systematic outline of their position are Pavlov (82), Watson (112), Guthrie (30), and Hull (38). The "field force" advocates include Köhler (53), Koffka (52), Tolman (108), Adams (1), and Wheeler (114).

A critical interpretation of the relative merits of these theories is beyond the scope or purpose of this paper. So far as the interpretation of neurological data is concerned all psychological theories of learning are classifiable into 2 groups: (1) theories postulating that all learning phenomena are of the same order—that qualitatively similar processes underlie *all* learning—and (2) theories which postulate several orders, varieties, or kinds of learning. Neither "conditioning" nor "insight" theories, taken singly, are adequate explanations of the learning process, since the "laws" that explain one type of learning do not operate, or operate only partially, in the explanation of other kinds of learning. Certain advocates of both the mechanistic and the field-force theories have taken cognizance of the difficulty of explaining all learning by the use of a single principle. Tolman (108), for example, in summarizing his position, recognizes 3 types of learning: conditioned reflex learning, trial and error learning, and inventive learning. He described the relationships existing between these as follows:

"This list distributes our laws under the three kinds of learning. All the laws involved in conditioned reflex learning are repeated under trial and error learning, and certain new ones are added. And all these are repeated under inventive learning and still other new ones added."

Although Hull (38, 39, 40) has attempted to explain all forms of learning in terms of conditioned reflexes, he has used the concept of conditioned reflex formed "habit hierarchies," to enable him to include apparently diverse kinds of learning phenomena within a single category.

The eclectic position of McDougall (74) is extremely interesting in regard to theories of levels of learning. Four different kinds of intelligent behavior are recognized by this authority. These are skilled movements<sup>4</sup>, the attachments of impulses to new objects<sup>5</sup>, perceptual acts, and imagination. Although McDougall's treatment makes free use of the much maligned mentalistic terms, it has certain advantages, since the human data bearing on the neurological correlates of learning and intelligence have been obtained principally from clinical material, which, despite all attempts at objectivity, is still not capable of being understood unless the validity of introspective data is granted.

### C. Categories of Learning

The treatment of the neuro-physiological correlates of learning is facilitated by distinguishing 4 fundamental varieties of learning: (1) non-voluntary or forced conditioning, (2) skills, (3) perceptual learning, and (4) conceptual or symbolic learning.<sup>6</sup>

(1) *Non-voluntary or Forced Conditioning.* The most primitive kind of learning may be designated as non-voluntary or forced conditioning. In this case learning takes place in a situation where the subject is not striving to acquire the modification, and where the subject may have no appreciation of the nature of the changes which

<sup>4</sup> Concerning the formation of skills McDougall writes as follows: "Now in acquiring a skilled movement (or secondarily automatic combination of movements) we undoubtedly build up in the nervous system a new motor mechanism similar to those motor mechanisms with which we are innately endowed. The process is, not only one of combination of preformed mechanisms; some analysis or breaking up of innate mechanisms into more elementary parts is also involved. . . ."

<sup>5</sup> After citing a hypothetical example of the attachment of an impulse to a new object, McDougall states, "This new reaction is what the mechanists call a 'conditioned reflex'."

<sup>6</sup> Tolman in a recent paper (109) presents the facts of learning under a four-fold classification which is similar to the one here advocated. The names of the analogous varieties of learning outlined by Tolman are: (1) conditioned reflex learning, (2) trial-and-error learning, (3) sign learning, and (4) higher forms of learning.

are occurring. This form of learning occurs often during periods of emotional stress.

Non-voluntary conditioning conforms closely to the conventional formulation of conditioned response schemata, inasmuch as a description of simultaneous (or near simultaneous) stimulus and response elements serves to give satisfactory indication of the conditions essential for the formation of the association.

Non-voluntary (forced) conditioning occurs often during periods of emotional stress, for the reason that emotion (especially fear, anger, and excitement) serves to misdirect attention or stultify the cognitive powers. Evidence for this view comes from 2 sources: (1) examples from psychiatric studies of non-voluntary conditioning in which repression (an act which results in lack of appreciation of the nature of the change) was a consequence of painful affect, and (2) the many commonly observed cases in which emotional states "paralyze" adaptive activities.

"Lack of appreciation" of the situation is derived not infrequently from 2 other sources: (1) the inability to comprehend the significance of a situation of undue complexity—especially frequently found in cases of animals, children, and the feeble-minded—, and (2) the lack of awareness of the existence or nature of a response being conditioned. Examples of the latter situation are to be found in the work of Menzies (75) on vasomotor conditioning and, with certain exceptions, in the work of Cason (12) on pupillary conditioning (i.e. cases where the responses are of the unconscious—involuntary—variety).

(2) *Skills*. The second variety of learning is the development of skills. The learning of skills consists essentially in the timing, the reorganization, and the automatization of the serial motor responses, both manual and verbal. Apraxia and expressive aphasia are complex human disorders in which skills are primarily affected. Skills may range from the comparatively simple to the complex; from those that are acquired automatically and relatively unconsciously on the part of the organism, to those which are acquired only after long striving (i.e. under the influence of "conation," "mental set," "habit-hierarchies," or "means-end-readinesses"). Skills differ from other varieties of learning in that they involve chiefly reorganization of systems underlying motor abilities.

(3) *Perceptual Learning*. Perceptual learning, as distinguished from non-voluntary conditioning, is characterized by the fact that the animal is consciously striving toward some goal. In perceptual activity the effort is directed, according to McDougall, toward "objects present to and affecting the senses." It may thus be differentiated from higher forms of learning which involve mental representation of objects and "inventive ideation." But no absolute differentiation can be made. Many learning situations, obviously,

involve both "perceptual" and "higher" forms of learning. Complicated sensory discriminations in animals are of the perceptual order. In man, disruption of perceptual organizations produces a condition of "imperception" or agnosia.<sup>7</sup>

(4) *Conceptual Learning or Symbolic Formulation.* The final variety of learning is that of conceptual learning, symbolic formulation, or inventive learning. There are several points of difference between this and perceptual learning, the most apparent being that conceptual learning or symbolic formulation involves mental representation, the capacity to think in terms of absent or "remote" objects. Tolman (108) has described learning of this class as a level of inventive ideation and has assigned to it not only the function of mental representation but also that of the ability to react appropriately to new elements in a situation that has never before confronted the organism. "Delayed reactions," "reasoning," "insight," and "hypothesizing" exemplify this order of function in the lower animals. This kind of learning capacity in man has been described under many captions such as that of Jackson—"ability to propositionize"; of Head—"capacity of symbolic formulation"; and of Goldstein—"capacity to differentiate figure from ground."

#### D. *The Nature of Intelligence*

The term intelligence refers to capacities of the organism (such as modifiability, capacity to perceive relationships, and the like), not to the process of modification *per se*. Thanks to the work of Spearman (100, 101), Thurstone (105), and Kelley (49), we are now attaining some degree of insight into the nature of these capacities and are no longer forced, when pressed for definition, to ignominiously offer the tautological definition, "intelligence is what the test tests." Kelley has already been able to define statistically certain factors of intelligence such as the verbal, the numerical, the spatial, and the mnemonic, and Weisenburg and McBride (113) have considered these problems in relation to the available data on aphasia. At present we can describe human intelligence more accurately than

<sup>7</sup> Word-blindness and word-deafness may be considered as perceptual disabilities, although they are not "simple perceptual" defects, since the present object is a word, but the word in turn is a symbol standing for a "remote" object. This type of activity, however, is simpler than the type of activity which we wish to classify as "conceptual or symbolic" learning. That such difficulties should be encountered is inevitable since no classification of levels of function can be absolute and free from overlapping.

we can intelligence in sub-human animals, since no satisfactory statistical analysis of the factors involved in animals other than man is yet available.

*E. Data Supporting Theories of Existence of Several Varieties of Learning*

Data concerning the nature and laws of learning, the anatomical organization of the nervous system and the effect of specific neural injury all lend support to the doctrine that various forms of learning exist. The following evidence is offered in support of this hypothesis.

(1) *Differences in Learning Laws and Learning Phenomena Indicate that Different Forms of Learning Exist.* The suddenness with which reorganization of behavior at the insight level occurs, as reported by Köhler (53), appears to be fundamentally different from the more gradual process of reorganization which has been reported as typical of trial-and-error learning by Thorndike (104), and of conditioned reflex learning as reported by Pavlov (82). This point, however, is contested by Guthrie (30).

Differences in the various laws of learning, as they apply to the several categories, have also been reported by Tolman (108), who writes as follows:

"All the laws involved in conditioned reflex learning are repeated under trial and error learning, and certain new ones are added. And all these are repeated under inventive learning and still other new ones added. It is to be remarked, however, that such a classification is probably not absolute."

Other learning phenomena also suggest levels of organization within the nervous system. Recently Harlow (31) has shown that non-voluntary conditioned responses remain after the unconditioned stimulus has been adapted out, but that this is not true for the more complicated learning situations. Settlage (98) has been able to prove that drugs have a differential influence on the qualitatively different levels of learning.

(2) *The Anatomical Structure of the Nervous System Suggests Different Levels of Organization.* This point of view is supported by the brilliant and extensive anatomical studies of Herrick (36) on the evolution of the central nervous systems. The following quotation from Herrick is particularly germane:

"The human cerebral cortex, then, exhibits three types of localization of function. The first is in the projection areas, which can be mapped in mosaic patterns on the surface. These are determined primarily by their respective



subcortical connections. Probably all parts of the cortex have some subcortical connections, but the principal systems of projection fibers are related with specific and clearly defined cortical areas. The second type is an expression of the architecture of the intracortical association systems. These stable arrangements of cell bodies and related association fibers constitute the apparatus of our standardized patterns of behavior and mental activity. This apparatus is present throughout the cortex. The functions performed by it are more or less permanently localized, but the pattern of this localization is very intricate and it cannot be projected upon the surface of the brain in mosaic designs like those of the projection centers. Third, in addition to the localized systems of projection and associational neurones, there is in the cortex an enormous mass of non-specific tissue. This is everywhere abundant; it varies in structure from place to place and in function from moment to moment. It integrates all cortical activities, synthesizes experience, and provides the plasticity of structure requisite for learning and higher mental processes in general."

The principle of anatomical levels has long since been stressed by Hughlings Jackson (43). Believing as he did that the higher neural levels constantly acted to regulate the activity in the lower levels, Jackson pointed out that in aphasia there is typically a sparing of emotional and automatic speech responses when the ability to "propositionize" was seriously affected or lost. Likewise, he emphasized the fact that a distinction existed between mere "imperception" and the ability to "propositionize."

(3) *Specific Neural Injury May Affect One Variety of Behavior and Allow Others to Remain Unaffected.* Countless examples may be cited from the data on aphasia and apraxia-agnosia in which the higher mental functions are lost when the lower are retained. This general principle was emphasized by Jackson and has been amply substantiated since. The recent studies of Brickner (6) and Penfield and Evans (83) on man, and the studies of Jacobsen and his co-workers (45, 46) on the monkey and chimpanzee have shown that extensive destruction of the frontal lobes (particularly when bilateral) causes loss of the most complex varieties of mental functioning (mental synthesis, immediate memory) whereas simpler processes are typically spared. These data might be explained, of course, on the basis of the compounding of simple units, such as conditioned reflexes. Such a theory, however, is faced with a very difficult problem in explaining how lower functions may be seriously affected when higher functions are spared. That this condition may exist is proved by a large body of data, both clinical and experimental. Verbal and manual skills, for example, may be abolished without serious loss to the higher mental processes. Campbell (11) in 1905,

correlated his anatomical studies with existing clinical data and concluded that the premotor area was the center for the integrating, organizing, and timing of fine adaptive movements, and that lesions in this area were associated with loss of these functions and sparing of higher intellectual functions. Jacobsen's (44) study has recently shown that following unilateral lesions of the premotor area in chimpanzees the patterns of response to the specific situation presented by problem boxes were lost and had to be relearned. The deficit occasioned by these lesions was not one of knowing what to do but of knowing how to go about doing it.

Verbal as well as manual skills may be lost when the higher mental functions are unaffected as is indicated by the following quotation from Weisenburg and McBride (113):

"In some cases, particularly in the simpler expressive disorders . . . the chief disturbances appear in the simpler mechanisms of speech production, the making of correct sounds or letter forms."

Even more striking are those cases in which simple speech skills are lost although the higher conceptual and symbolic processes remain. Again quoting from Weisenburg and McBride (113):

"There are patients, usually of the receptive type, who cannot evoke a necessary word but by paraphrasing give a fairly adequate substitute for it. A similar situation appears in the case of those patients who cannot tell the product of two numbers, although this figure has come to them more or less automatically ever since they learned the multiplication tables, and yet they can work out a system for making new determination of the product."

## II. THE PROBLEM OF ENCEPHALIZATION AND "CORTICALIZATION"

### A. *General Nature of the Problem*

The principle of encephalization has long been recognized and studied both by anatomists and physiologists. According to Brouwer (8), "Many functions having their anatomical basis in primitive animals in lower parts of the nervous system are taken over in the ascending scale by the forebrain." Similarly, Dusser de Barenne (15) states that "Functional encephalization is the underlying principle of the progressive development of the central nervous system which we observe in the animal scale." Since encephalization refers to "the shift of functional dominance from the spinal cord up through all levels of the nervous system" Dusser de Barenne (15) has recently coined the term "corticalization" to refer to that stage of the process of encephalization in which functions

originally localized within lower cerebral centers are transferred to the cerebral cortex. "Corticalization" is closely linked with the problem of location of functional areas in the cortex and is characterized by (1) "concentration of functions" within limited cortical areas, and (2) the tendency for a given function to be mediated strictly and entirely by a cortical center. In the case of vision, for example, certain functions which in the lower mammals are mediated by both cortical and subcortical centers, are mediated in man by the cortex alone.

The anatomical data dealing with the problem of encephalization and "corticalization" have been discussed by Herrick (36), Tilney and Riley (107), and Tilney (106).

### B. "Corticalization"

(1) "Corticalization" of Motor Functions. The progressive "corticalization" of motor functions is indicated by studies made on decorticate animals representative of different levels in the phylogenetic scale and by studies on anencephalic children. Data

TABLE I  
POSTURAL RESPONSES IN THE ABSENCE OF THE CEREBRAL CORTX

<i>Animal</i>	<i>Observable Loss</i>	<i>Source</i>
Frog	None	Schrader (95)
Pigeon	None	Martin (73)
Rat	"Rarely assumed abnormal leg attitudes"	Bard <sup>a</sup> (2)
Cat	"Fairly normal posture, progression"	De Barenne (15)
Dog	Standing, walking possible.	De Barenne (15)
Monkey	Sit with help, no progression	Karplus (48)
Man	Almost complete loss	Edinger (17)

bearing on this problem are presented schematically in Table I, these data being adapted in part from material discussed by Dusser de Barenne (15).

These data are supplemented by the many observations made by Fulton (21, 22) and his co-workers on the effect of unilateral and bilateral lesions in the motor and premotor areas. Without exception they have found that the loss resulting from comparable lesions becomes more severe as we ascend the primate order.

<sup>a</sup> Bard and Brooks (2) have shown that complicated postural reactions, the placing and hopping reactions, are represented in strictly localized areas in the rat, and are dependent upon the integrity of a specific fraction of the cortex in the cat. Following Rademaker (90), Bard and Brooks point out that the placing reactions may be conditioned or learned responses.

(2) "*Corticalization*" of *Visual Functions*. The problem of the "corticalization" of the visual functions has already been critically reviewed by Marquis (72). Data on the cerebral localization of visual functions in various animals have also been discussed critically by Lashley (61), Brown (9), Mettler (76), and Dusser de Barenne (15). Certain of these data are presented briefly in Table II.

TABLE II

## VISUAL RESPONSES IN THE ABSENCE OF THE CORTICAL RECEPTIVE AREA

<i>Animal</i>	<i>Observable Loss</i>	<i>Source</i>
Frog	None	Shrader (95)
Pigeon	Some, degree undetermined	Schrader (96)
Rat	Threshold for brightness raised	Lashley (61)
Rat	Detailed pattern vision lost	Lashley (61)
Cat	Threshold for brightness raised	Smith (99)
Cat	Detailed pattern vision lost	Smith (99)
Dog	Threshold for brightness raised	Marquis (72)
Dog	Detailed pattern vision lost	Marquis (72)
Monkey	Brightness discrimination partially retained <sup>9</sup>	Brown (10)
Monkey	Detailed pattern vision lost	Brown (10)
Man	All visual sensations lost <sup>10</sup>	Marquis (72)

(3) "*Corticalization*" of *Other Sensory Functions*. The data on the degree of "corticalization" of the other sensory modalities have not been worked out in such detail as have those on the "corticalization" of visual functions. The interested reader may be referred to Dusser de Barenne (15) Mettler (76), and Brown (9) for additional material.

The available evidence indicates that no other sensory modality has undergone functional encephalization as completely as has vision and that very probably, even in man, vision is the only sensory analyzer to show complete functional "corticalization."

Since Edinger and Fischer (17) have shown that anencephalic children make strong negative reactions toward unpleasant tastes

<sup>9</sup> Marquis has shown that wink responses in the monkey, conditioned to a visual stimulus, were retained after bilateral extirpation of the occipital lobe; and Brown and Schäfer (10) reported that after complete bilateral removal of the occipital lobe in apes, grasping motions were made by the animal toward objects held between it and a source of light. Klüver (51) has recently shown that the monkey, following bilateral occipital lobectomy, can still make discriminations in the quantity of luminous flux entering the eyes.

<sup>10</sup> The effect of complete destruction of the striate area on conscious perception of visual stimuli in man has long been argued. Data bearing on this question are summarized by Marquis who concludes that, in all probability, all conscious sensations are lost.

and odors, there can be little doubt that the functions are subserved by subcortical centers. Likewise, Head (33) has shown that the crude body sensations (pressure, extremes of temperature, roughness, pleasantness, and unpleasantness) are localized in the thalamus.

The evidence for audition is not so clear, but according to Monakow (cited by Dusser de Barenne, 15) there is evidence that some traces of audition remain after bilateral destruction of the auditory cortical projection area.

(4) "*Corticalization*" of *Learning and Intelligence*. There can be no reasonable doubt that the principles of encephalization, so clearly demonstrated for the motor and sensory functions, also apply

TABLE III

## CONDITIONED RESPONSES IN THE ABSENCE OF THE CEREBRAL CORTEX

<i>Animal</i>	<i>Observable Loss</i>	<i>Source</i>
Goldfish	Little, if any, loss	Sears (97)
Pigeon	Transient loss of memory	Martin (73)
Pigeon	Conditioned responses readily formed	Beritoff (4)
Rat	Conditioned responses formed	Jellinek (47)
Dog	"Generalized" conditioned responses	Culler (13)
Monkey	No evidence for conditioning	Karplus (48)
Man	No evidence for conditioning	Edinger (17)

to the higher mental processes. The following summary of the effect of decortication on the conditioning process in various animals is presented in Table III.

(5) "*Concentration of Representation*" in the *Cerebral Cortex*. As "corticalization" of a function becomes more complete, there is a tendency for its localization to become more precise; thus a function which may originally have been represented throughout a considerable part of the surface of the cortex, later becomes localized within a much more restricted cortical area. Grünbaum and Sherrington (29) and Fulton and Keller (21) have shown that fine, highly coördinated movements become localized more precisely within the motor cortex as the primate order is ascended.<sup>11</sup>

There is also every reason to believe that "concentration of representation" of the receptive as well as the motor centers of the cortex takes place. Ruch and Fulton (91)<sup>12</sup> in their study of the

<sup>11</sup> Likewise, as the phylogenetic scale is descended, there is a decrease in the "concentration of representation," as indicated for motor functions by experiments on the electrical stimulation of the motor cortex in dogs, cats, rats, opossums, and alligators.

<sup>12</sup> As far as the present author knows, the expression, "concentration of representation," was coined by Ruch and Fulton to cover the phenomenon described.



somatic sensory function of the cerebral cortex in monkey and chimpanzee, conclude that: "These observations point to a progressively greater degree of 'encephalization' and 'concentration of representation' of somatic sensory function in the primate series."

Although the data are not so clear there can be little doubt that certain of the neuro-physiological mechanisms underlying learning and intelligence have also undergone "concentration of representation," and that this principle is of considerable importance in explaining contradictory results obtained by various investigators. The parareceptive areas appear to be much more precisely localized in the higher than in the lower mammals, and if the results of comparative work are not viewed in the light of this principle, there must inevitably be contradictory appearances. Again, in man as opposed to lower animals, greater specificity of intellectual loss following extirpation of the frontal lobes is very possibly the result of "concentration of representation" of specific neural processes which facilitate or synthesize complicated mental processes.<sup>13</sup>

(6) *Shift from Bilateral to Unilateral Representation of Motility and Sensibility Within a Single Hemisphere.* In the lower animals there are indications of a bilateral representation of motility and sensibility within each of the cerebral hemispheres. As we pass from the cat and the dog to the lower primates and finally to man, there is a tendency for the bilateral representation of motility to change to an ever increasing unilaterality of representation. This process is not complete in monkey, gibbon, or chimpanzee, as indicated by the work of Fulton and Keller (21), and in all probability is not complete in man, since both Dandy (14) and Gardner (24) have shown that voluntary movements on the contralateral side are possible after total extirpation of the right cerebral hemisphere.

Unilateral representation of the more discriminative aspects of the somesthetic functions is apparently complete in man (excluding the face area) as is proved by the studies of Head (33), Dandy (14) and Gardner (24). In animals as high in the phylogenetic scale as the monkey and chimpanzee, however, this process is not complete, as the work of Ruch and Fulton (91), Harlow and Settlage (32), and Dusser de Barenne (15) seems to indicate. In all primates

<sup>13</sup> Comparison may be made of the effect of frontal lobe lesions in man (Brickner, 6), and monkey and ape (Jacobsen, 45), with the effect of the same operation on the dog (Pavlov, 82), or the rat (Loucks, 65). See also Bianchi (5) for earlier work on the effect of frontal lobe lesions.

above the lemur and tarsier, the right and left visual fields are localized exclusively within the contralateral hemisphere of the cerebral cortex, save possibly for a small macular area (see Piéron, 88, Fox and German, 20, Penfield and Evans, 85, and Poliak, 89).

### C. Hemispherical Dominance

In the preceding sections the writer pointed out that the process of "corticalization" is indicated by increasing "concentration of representation" and also by a tendency toward unilateral representation of motor and sensory functions within a single hemisphere, and it is more than possible that these processes represent steps toward true hemispherical dominance. Whether or not this is true, the cortical organization of man differs from that of lower animals (with the possible exception of the anthropoid apes) in that many of the higher psychological activities are located predominantly or entirely within a single hemisphere.

In man the left hemisphere is usually dominant and thus determines to a large degree the handedness, eyedness, and language functions of the individual. That handedness may be used as an indicator of cerebral dominance is shown by the following quotation from Weisenburg and McBride (113):

"Thus among the 92 clearly right-handed or clearly left-handed patients there are only 4 exceptions to the rule that aphasia appears with lesions of the dominant hemisphere as indicated by handedness, and fails to appear with lesions of the non-dominant hemisphere. In other words, the dominance indicated by handedness is a criterion of the crucial hemisphere for speech in about 95 per cent of the cases."

It is generally assumed that hemispherical dominance is determined primarily by inherited factors. Studies of handedness of rats by Yoshioka (116) and Tsai and Maurer (111) lend support to this hypothesis. Miles (77) has shown that eyedness tends to remain constant throughout the life of the adult regardless of special training.

There are data, however, to support the idea that in certain cases transfer of cerebral dominance may occur in whole or in part. Of especial interest in this regard is the study by Lowell, Waggoner, and Kahn (66). In the case they reported, aphasia resulted from injury to the region of the *right* angular gyrus in a boy who had been originally right-handed and right-eyed, but who, following amputation of the right hand at the age of 10, had learned to write with his left hand and had become left-eyed. German and Fox (25) also point

out, in one of their cases, the possibility of the occurrence of such transfer.

Abundant clinical data supports the hypothesis that incomplete or conflicting cortical dominance may cause serious language disorders in otherwise normal individuals. This very difficult problem has been critically reviewed by Orton (81) and by Travis (110).

(1) *Hemispherical Dominance and Speech.* It is an uncontested fact that speech functions are predominantly localized in the dominant hemisphere. Both Dandy (14) and Gardner (24) report cases of extirpation of the entire right hemisphere of right-handed patients without serious speech loss.<sup>14</sup>

The relationships between injury of the right hemisphere and aphasia are summarized by Weisenburg and McBride as follows:

"It would seem as if the right brain, while not directly concerned in language in the right-handed individual, nevertheless is in a state of receptivity for language acquisition, the degree varying in accordance with the use of the left hand in writing. In addition, non-language functions and behavior, which are almost always implicated in aphasia, have admittedly a bilateral cerebral basis. . . . There is nothing to show that the right brain has any specific language function as indicated by Jackson and some more recent investigators."

As indicated, other investigators have ascribed to different views. Henschen (35) and Kleist (50) have both maintained that following injury to the dominant hemisphere, the non-dominant hemisphere can take over certain limited language functions.

If the language functions are entirely dependent upon the integrity of the dominant hemisphere, the psychological observations of Sears and Snedden (case 4, reported by German, Fox and others, 25) are difficult to explain. In this case, despite practically complete ablation of areas 17, 18, and 19, and injury to the posterior portion of 7, 37, and 39 of the dominant hemisphere, complete word-blindness did not appear, although the loss was great. Since it is doubtful that this sparing of comprehension can be attributed to either the normal or vicarious functioning of the remaining left visual cortex, or of the more anterior cortical areas, the possibility of these activities being mediated by the visual areas of the non-dominant hemisphere cannot be ignored.

<sup>14</sup> It seems unlikely that such a large mass of cortical tissue could be excised without some loss, even if slight. Actual data presented by Dandy suggest loss of intelligence and certain release symptoms including the tell-tale "Witzelsucht." The psychometric examinations were, unfortunately, entirely inadequate. Data presented by Weisenburg and McBride (113, pp. 324 and 328) indicate that there is language loss following right-sided lesions.

Recently Zollinger (117) has reported a very remarkable case of a right-handed woman who survived 17 days after removal of the left cerebral hemisphere. Rudimentary language responses were retained. This finding proves that the more automatic speech functions are functionally localized in both hemispheres, or that they can be transferred to the non-dominant hemisphere following damage to or excision of the dominant hemisphere.

(2) *Hemispherical Dominance and Non-Language Functions.* The relationship of non-language functions to hemispherical dominance has been given scant consideration in contrast to the large amount of work on hemispherical dominance and aphasia. As Weisenburg and McBride (113) suggest, agnosic and apraxic disturbances are typically the result of widespread bilateral lesions. Certain complex non-language functions, however, show a marked degree of hemispherical dominance.

Thus Schilder (93) has presented evidence that complex gnosic functions, especially those relating to the "body image," are located within the dominant hemisphere; indeed, this author states categorically that "There is no question that the dominance of the body image of the left hemisphere exists," and, "The body image is a 'unit of its own' and is not the 'sum of optic and tactile sensations and images.'" Body image imperception is primarily characterized by helplessness on the part of the patient to point to parts of his own body.

Pick (86) has also described such a condition and has given it the name of autotopognosia. Gerstmann's (26) syndrome may be considered to be partial loss of body image perception<sup>18</sup> and is characterized by inability in the patient to recognize, name and show his fingers, imitate finger posture of others, orient himself with relation to right and left, and to write and calculate efficiently.

Finger agnosia results typically from lesions in the angular gyrus, body image agnosia being caused by injury to the neighboring parts of the parietal lobe, especially the lower parietal lobe.

Schilder and Stengel (94) have described an interesting condition designated as pain asymbolia and characterized by inability on the part of the patient to appreciate the significance of unconditioned or conditioned pain stimuli. Lesions responsible for this condition are localized in the anterior and basal parts of the dominant supra-marginal gyrus.

<sup>18</sup> Schilder (93, p. 470) states that "Finger agnosia is thus far the only isolated agnostic disturbance concerning the body image that is known."

Of special psychological importance are cases of anosognosia, characterized either by non-perception of one's own paralysis in *left sided hemiplegia* or by denial of ownership of the existence of the paralyzed *left* side of the body. This disorder is caused by lesions lying in the inferior parietal lobe of the *non-dominant* hemisphere. A very ingenious explanation of this disorder, has been given by Schilder (93) who has also summarized the pertinent literature bearing on the problem. A patient is much more willing to deny ownership of identity of the left side of the body since "We live more intensively with the right side of our body than the left side." Thus, "We deal with a psychic-attitude which is very similar to the attitude of repression, and still we can trace it back to a localized lesion of the brain."

### III. NEURO-PHYSIOLOGICAL THEORIES OF INTELLIGENCE

The neuro-physiological theories of learning and intelligence may be differentiated into 3 fundamentally different types:<sup>16</sup> (1) the agglutination theories; (2) the theories of non-specificity, or dynamic theories; and (3) theories of hierarchical synthesizing or associating centers. It should be noted, however, that the position of many authors represents a compromise among these 3 positions.

#### A. *The Agglutination or Sensory Aggregate Theories*

Intelligence, as conceived by the exponents of agglutination theories, is the sum total of the activities of the various cortical centers. This position would appear to be indicated for all association theories, whether the association is one of sensory images, or association between stimuli and responses.

Munk (80) was an ardent exponent of this position. His theories have been summarized by Lashley (56) as follows:

"Munk divided the cerebrum into a number of primary 'sensory spheres'—visual, auditory, tactile, etc.—in each of which he believed the images and ideas associated with a single mode of sensation are stored and elaborated. Intelligence was conceived as the aggregate of all these products of the single sensory spheres, brought together by the manifold interconnections between them."

Although Munk's position is extreme, especially in denying the existence of any specialized association centers, there have been many other advocates of agglutination theories or theories of sensory aggregates both before and since. The hypothetical association

<sup>16</sup> This classification is a modification of an earlier classification given by Lashley (56) in reviewing similar data.



pathways connecting visual, auditory, "glosso-" and "cheiro-kinaesthetic" centers as outlined by Bastian (3) show the prominent part that agglutination theories played in the aphasia theories of the "diagram-makers."<sup>17</sup>

Kleist (50) and Henschen (35) have interpreted the data of aphasia principally in terms of an agglutination theory. Schaller (92) quotes Henschen as believing that:

"Speech centers with sharp localization are the workshops of ideation (Begriffe). Ideation depends on concepts (Vorstellungen) of different associated auditory, optic, tactile, etc., nature. . . . Brain centers are banded together, so that when one is affected, the whole may be affected; thus in forms of aphasia, word blindness may occasion word agraphia, etc."

Pavlov (82) also subscribes to an agglutination theory, since thinking is explained in terms of conditioned reflexes; and conditioned reflexes are either association between a "sensory analyzer" and a response center, or between two different "sensory analyzers." In the study of the neurological correlates of conditioning, Pavlov attempted the destruction of the centers for each of these analyzers, and the results he reports is that the loss is specific for the analyzer affected.

According to those who advocate some form of agglutination theory, loss of intellectual ability may be occasioned either by destruction within a sensory sphere or by interruption of the association pathways between sensory spheres. The latter type of loss is described in human cases as transcortical or conduction aphasia.

Support for agglutination theories is given by the fact that lesions limited to a single "sensory sphere" bring about far greater loss in those learned organizations related to the affected sensory modality than to those related to the other modalities. Thus Pavlov reports that lesions in the occipital pole disrupt visual conditioned reflexes but do not affect auditory or cutaneous conditioned reflexes. Likewise, there are many striking examples from human data, as in the case of visual imperception reported by Von Stauffenberg (103) as summarized by Head (34, pp. 106-110) in which the patient

". . . could not recognize with certainty people, objects or pictures and yet her intelligence and memory remained fairly good throughout. . . . As might

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<sup>17</sup> Certain of the "diagram-makers" departed from a strict agglutination theory in that they postulated a "center of ideation" as did Broadbent (7) and Charcot, whose diagram is given by Piéron (88, p. 151). The belief is pure word blindness, pure word-deafness, and kinaesthetic word-centers testifies to the emphasis given to agglutination theory.

have been expected, she could not read even her own name with certainty. She was unable to decipher letters or syllables written by herself. . . ."

At the same time,

"Hearing remained intact and, . . . auditory perception was unaffected. She reacted to the slightest sound, recognized running water and, when she heard whistling, said it was like the noise made by the tramcar and locomotive. She could sing well-known melodies correctly and finally her hearing became so acute as to form a valuable aid to recognition in daily life. Taste and smell were unaffected."

Almost as striking is a case of auditory imperception reported by Henschen (35), and summarized by Head (34, pp. 113-114):

" . . . she was not deaf and could then appreciate all tones from the highest to the lowest. . . . She was able to appreciate the difference between voices and, although she was 'not completely word-deaf to a certain kind of words,' she had great difficulty in understanding what was said to her. . . . Told to put out her tongue, she repeated the order aloud and then executed it correctly [*aid of proprioceptive cues?*]. She seemed to understand what was said to her more readily if she had asked the question herself. . . . Printed and written matter was now understood and there was no 'alexia.'"

— Since loss is never limited to the sensory modality affected, particularly as regards the most complicated intellectual processes, this might be taken as an argument against a strict interpretation in terms of agglutination theories. On the other hand, it can be argued that if intelligence is the sum total of the activities of the "sensory spheres," extended loss in a single sphere would be expected to affect general intelligence.

#### B. *Theories of Non-Specificity, or Dynamic Theories*

Belief that the intellectual functions of the cerebral cortex have no specific localization was first given experimental support by the work of Flourens (19). This investigator concluded from experiments on pigeons that "All perceptions and volitions have the same distribution in the hemispheres."

Coming at a time when many students were reacting against Gall's extreme position of localization, Flourens's view attracted widespread attention and was given due consideration. The localization of Broca's area, the motor area, and the various sensory areas, however, gradually led the majority of investigators to accept some theory of localization, even for the higher intellectual processes.

In spite of this fact Goltz (28) stressed the relationship between amount of destruction and loss of intelligence for bilateral lesions

and Loeb (64) held to the view that "associative" memory could not be restricted to any part of the cortex.

Valuable as these early contributions may be, the work of Lashley (56) is of preëminent importance as forming a basis for non-specific or dynamic theories of intelligence.<sup>18</sup>

The most striking of the principles of non-specificity advanced by Lashley was that of equipotentiality. Concerning this Lashley (56) has written as follows:

"The term 'equipotentiality' I have used to designate the apparent capacity of any intact part of a functional area to carry out, with or without reduction in efficiency, the functions which are lost by destruction of the whole. This capacity varies from one area to another and with the character of the functions involved. It probably holds only for the association areas and for functions more complex than simple sensitivity or motor co-ordination."

The data from which this principle was derived were obtained in large part from the study of the rat on the maze. Lashley, however, believed that the principle could be extended, for he writes as follows, in the same article from which the above quotation was taken:

"Analysis of the maze habit indicates that its formation involves processes which are characteristic of intelligent behavior. Hence the results for the rat are generalized for cerebral function in intelligence. Data on dementia in man are suggestive of conditions similar to those found after cerebral injury in the rat."

Furthermore, Lashley (58) attempts to strengthen his position by the use of certain clinical data.

He writes:

"In several reviews I have called attention to the fact that the characteristics of aphasia, visual agnosia, and the like in man are far better fitted by a

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<sup>18</sup> Lashley does not deny cortical localization of sensory or motor functions. His "dynamic" principles apply only to intelligence or integration, and his position has been widely misunderstood, not only by neurologists but also by psychologists. An explanation of his position is given in a communication to Mettler (76) as follows:

"The fact of specialization of cortical structures in the rat is obvious. I have just published evidence for a very detailed projection of the retina upon the area striata and am accumulating similar evidence for the auditory and somesthetic areas. . . . I have never questioned the existence of cortical sensory and motor fields with special functions, but to me this is only one of a number of important clues to the mechanism of integration and one which is likely to be very misleading unless it is considered in relation to other facts."

Herrick (37) and Hunter (42) have pointed out the fact that in certain earlier communications Lashley (55) and Franz and Lashley (62) were not so careful to restrict "equipotentiality" of function to the integrative mechanisms.

theory of mass action than by any theory of restricted sensorimotor paths. These symptoms follow lesions outside of the direct sensory projection fields, they show global deterioration of function, and there is some agreement among clinicians that their severity is proportional to the magnitude of the lesion."

In the interpretation of his data Lashley (56) quite definitely rules out any explanation in terms of agglutination theory. This is obvious from the following quotation:

"We . . . have evidence that the maze habit is not interfered with by any purely sensory or motor defect, that the formation of a sensory habit is not retarded by absence of the corresponding cortical sensory area, and that the deterioration following lesions in different cortical sensory fields is qualitatively the same for all fields. All of this points to the conclusion that defects of the maze habit are due to some general deterioration, which affects the associative mechanism as a whole rather than distinct, qualitatively different, elements of the habit. At least it seems certain that if the deterioration is specific, it is not so for any function which can be stated in terms of simple sensorimotor adjustment."

Similar to the hypothesis of "equipotentiality" is that of "areal equipotentiality" which implies that the associative function is mediated with equal effectiveness by any part of a single functional area. Concerning this Lashley (59) wrote that "He [Lashley] also presented some evidence that, with lesions restricted to any single cortical area, the retardation in maze learning is proportional to the amount of destruction within that area"; but he qualified this statement by saying that the data of Lashley and Wiley (63), "are less consistent on this point but support it for certain areas."

Study of the relationships between cortical mass and association within a single functional field (particularly the study of brightness discrimination following lesions of area w of Fortuyn) led Lashley to postulate the principle of mass action,<sup>19</sup> which he has defined as ". . . reduction in efficiency of performance after cerebral lesions, but without evidence for qualitative differences in performances for lesions within a given field." (See Mettler, 76, p. 397.) Previously Lashley (58) had written:

"If the habit mechanism (for brightness discrimination) is subcortical, its functioning is still conditioned by the quantity of tissue intact in field w, irre-

<sup>19</sup> In 1929 Lashley (56) had expressed himself as follows concerning "mass function": "I have already given evidence (1927) which is augmented in the present study, that the equipotentiality is not absolute but is subject to a law of mass action whereby the efficiency of performance of an entire complex function may be reduced in proportion to the extent of brain injury within an area whose parts are not more specialized for one component of the function than for another."

spective of the locus of the tissue within the field. It was to explain these facts that I first postulated cerebral mass action."

Investigation of the problem of mass action led Lashley and co-workers into a detailed study of the visual system in the rat. Lashley (61) has recently summarized as follows his position as regards the visual system in the rat:

"The available data suggest an equivalence of function for brightness vision between the striate cortex and the superior colliculi. Either system may be destroyed without abolishing the capacity to react to brightness. Before we can accept this equivalence, however, more must be learned concerning the functions of the optic thalamus."

There have been many criticisms of the interpretation that Lashley has given to his work, especially to his principle of "equipotentiality." One of the most cogent is that made by Hunter (41), who has suggested that the results obtained on the maze might be explained as being due to the fact that "there are many sensory projection areas<sup>20</sup> involved in maze control and that the lesions do not affect all of them."

Lashley (58), replying to Hunter, has denied that the results can be explained solely on the basis of the sensory systems involved since, "... the destruction of the sensory projection area produced a disturbance of function from 9 to 60 times as great as did the destruction of the corresponding sensory mechanism." Therefore Lashley concludes that:

"On the basis of the evidence reviewed . . . , I must conclude that the theory of sense privation as an explanation of the relation between the extent of the lesion and deficiency of performance is inadequate to account for the facts observed. The hypothesis which seems best in accord with the experimental results is that, for certain types of behavior, the projection areas have, in addition to their specific sensory or motor functions, a nonspecific (perhaps facilitative) function in which they are equipotential."

Lashley's appeal to the difference in the effect of destruction of the receptor and of destruction in the receptive-parareceptive areas, in this Lashley-Hunter controversy, is not entirely pertinent. The distinction between peripheral blindness and deafness and "mind blindness and deafness" has long been made. Furthermore, the

<sup>20</sup> The term "sensory sphere," "analyzer," or "receptive-parareceptive area" might be preferable to projection area since it is doubtful if Hunter wishes to imply that only projection areas are affected. Hunter's alternate explanation of Lashley's results may be looked upon as an explanation in terms of an agglutination theory.



advocates of agglutination theories have always insisted that the associations are made between the "sensory spheres," not between receptors. Under the circumstances Hunter's (42) reply is justified:

"Where a portion of the cortex is removed and so where integration is interfered with, the loss is greater than where the cortex is left intact and the receptor removed. These facts, however, bear neither upon equipotentiality nor upon mass action (unless mass action equals integration)."

Hunter's critical analysis of Lashley's work does not, however, prove that the data on the learning of the maze are *best* explained in terms of an agglutination theory; they only show that with analysis of the results as they now stand, the maze learning data *may* be explained in terms of integration between the cortical receptive-parareceptive centers.

Lashley has recently been criticized by Mettler (76) on another count, namely, that equipotentiality violates "the morphological principles of organization which have followed the development of the neurone doctrine." In more detail Mettler (76) writes:

"Equipotentiality presupposes a homogeneity of the cortex which does not exist. It is a little difficult to decide how close Lashley's theory of mass-action comes to equipotentiality.

". . . No fault can be found with any theory which involves modifiability of cortical interrelations or proposes plasticity as a characteristic of cerebral processes. It must however be realized that such modifiability as may occur is conditioned by previous structure and that certain types of modifiability are, to say the least, highly improbable."

Mettler is unquestionably right in insisting on the importance of recognizing basic principles of structural organization in the formulation of any theory of mental functioning. His criticism may well apply to such theoretical interpretations as the following suggestion of Lashley's (56):

"Applying these postulates [suggested by Kappers] to the problem of discrimination reaction, we may assume that a given ratio of stimulus intensities on two peripheral points establishes a potential difference between two corresponding points on the cortex. The direction of polarization and the steepness of gradient will remain constant in spite of considerable alterations in the absolute positions of the centers of excitation. Cells subject to the influence of this system will have their excitability modified in a constant direction so long as the properties of the gradient field are constant."

The degree to which the "morphological principles of organization" prove or disprove the existence of circumscribed or non-circumscribed functional cortical areas remains to be determined. As

we have already pointed out, outstanding workers in neuro-anatomy have suggested that the higher mental activities need not be closely localized; and the less closely a particular function is localized, the more closely we approach "equipotentiality" of that particular function.

In various reviews Lashley (56, 57, 59) has insisted that clinical data support his position, and has cited the work of Head (34), Piéron (88), Goldstein (27) and others in support of this contention. Both Stagner (102) and Mettler (76) have pointed out that Head (34) is a proponent of the doctrine of localization. The same may be shown to be true of both Piéron (88) and Goldstein (27). By using selected paragraphs from the work of any of these workers it is possible to convey the implication that they believe either in localization or in non-specificity of function. All of these workers agree that a higher degree of functional localization pertains to the simpler than to the more complex functions; while at the same time asserting that there is some form of localization even at such a complex level of integration as that involved in "symbolic formulation" and "differentiation of figure from ground." Thus Head (34) sought to localize roughly 4 "centers" involved in "symbolic formulation." Goldstein's views are adequately summarized by Weisenburg as follows: "Goldstein<sup>21</sup> thinks of localization as a definite distribution of excitation within a structurally differentiated nervous system with a particular structuration of the excitation in a definite area."

Among the modern neurologists Schilder (93) and German and Fox (25) deny the validity of Lashley's position, and even so vigorous a critic of strict localization as Weisenburg, states that:

"The extent of the lesion is by no means the sole determinant of the extent of the disorder. Such is the complexity of cerebral functioning in relation to language and thinking that a slight lesion, particularly if it affects one of many crucial areas, may lead to extensive aphasic disturbances."

In fairness to Lashley it must be pointed out that certain of his critics have apparently overlooked his contention that equipotentiality exists only for the more complicated mental processes.

Various attempts have been made to determine the nature of the particular mental processes affected by cerebral lesions in lower

<sup>21</sup> Goldstein's views are more "dynamic" than those of any other modern clinician. Yet Goldstein's firm belief in localization is indicated by the fact that he recognizes peripheral, transcortical, and central motor aphasia, pure word-deafness, cortical sensory aphasia, central aphasia, and amnesic aphasia.

animals. Krechevsky (54) has shown that "hypotheses" of rats decrease in number and efficiency following these lesions. Maier (67, 68, 69) has studied the effect of cortical destruction on "reasoning" and has found that the effect cannot be entirely explained by equipotentiality. Lashley (60) has recently studied the effect of cerebral lesions on the ability of rats to learn to solve problem boxes. His analyses of the data, including observations, indicated that the loss was to be explained as follows:

"The learning of such problems at normal rate indicates that the mechanism of association, as such, is not disturbed by cerebral lesions and that retardation from cerebral lesions is due rather to disturbance of such functions as are implied by the terms attention, insight, and initiative."

Lashley was able to show that rats, following extirpations of 20% or more of the cerebral cortex, irrespective of the cortical field involved, showed limited exploratory ability; and that cerebral lesions were followed by "a lack of aggressiveness toward elements of the problem situation,"<sup>22</sup> and the reactions of the operated rats to the various objects in the problem situation, "lack the variety and adaptive character of the behavior of the normal animal." Finally Lashley notes that the behavior of animals with cerebral lesions "is more stereotyped in that they tend to repeat the movements which were first successful in opening the boxes, instead of modifying them to conform to the actual requirements of the latch" whereas normal animals seem to "dissociate the latch from other parts of the problem situation and later react to it as an object to be manipulated in a certain way."

As Lashley has indicated, loss of mental capacity of these kinds has frequently been reported following widespread cortical injuries in man.

Concerning his interpretations of his own data Lashley (60) writes as follows:

"In previous discussions of the effects of cerebral lesions I have striven to avoid such psychological interpretations of behavior. They do not give us any

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<sup>22</sup> Lashley (60, p. 32) has also shown that following extensive cortical lesions there is frequently great difficulty in obtaining limens, suggesting fluctuating attention rather than a genuine disturbance in vision for brightness. Head (33) found exactly the same difficulty in obtaining limens to body sense stimuli following injury to the parietal lobe. Also the subjects frequently had "hallucinatory" images. It should be noted that in the case of Head's subjects the "attention" was largely limited to the sense modality whose receptive-parareceptive area was injured.

understanding of the cerebral mechanisms involved and tend to obscure the issues by presenting a pseudoscientific explanation in terms of empathy. At best they merely serve to classify the behavior with certain categories of human experience, for which we have no physiological explanation. But my efforts to confine discussion to a physiological level seem to have led to serious misunderstandings of my position. Conceptions of non-localized undifferentiated functions of the cerebrum have been rejected by writers who use a psychological terminology implying precisely the same thing. I therefore present the above psychological interpretations, not because they have any explanatory value, but in order to make the dynamic concepts of cerebral function sound less strange by identifying them with terms which, though scientifically meaningless, are familiar."

The present writer sharply disagrees with Lashley. The attempt to analyze any performance would seem to be commendable even if the psychological interpretations have at the present time no known "physiological foundation." Such classification at least indicates the problem, even if it leads to no immediate solution, and the neuro-physiological correlates of psychological terms are as well known as are the neuro-physiological correlates of "mass action," "equipotentiality," "intelligence," "maze learning," "problem box solution," or "potential gradient." The value of terminology even at a "physiological level" which leads to widespread "serious misunderstandings" among highly competent authorities is doubtful.

### *C. Theories of Hierarchical Synthesizing or Associating Centers*

The assumption that hierarchical synthesizing or associating centers exist is not of necessity opposed to the theories already presented. Certain of the authorities who have emphasized the importance of sensory association have postulated a higher center controlling or determining in part or in whole the most complicated mental processes. This is the position of Henschen who is quoted by Schaller (92) as believing:

"The number of stages to completion of word ideation from the auditory or optic word is not known. There are probably individual variations, but in the last the frontal lobe, and especially the left, is indicated as the final stage."

Similarly Brickner (6) has interpreted the functions of the frontal lobe as being that of "intellectual synthesis." He writes:

". . . intellectual impairment begins on a higher level than that, for example, of visuo-auditory association. It first appears somewhere in the domain of the complex synthesis of those materials first appreciated in the more caudal parts of the brain. Moreover, the intellectual impairment seems to include the synthesis of *all* the materials accumulated posteriorly: there is

diminution in the amount of synthesis relative to *every* process. [Italics are Brickner's.] . . . Each activity and process entered into by all the other regions of the cortex must also be represented in the frontal cortex. There, elaborate association and synthesis must occur between all of these other, more posteriorly initiated processes, resulting in the production of our most complex intellectual activity."

On the other hand Herrick (36) has outlined a theory of the nature and origin of higher associative centers which suggests that these need not be localized in a single part of the cortex but may be widespread throughout its surface. If his hypothesis is valid, the higher mental processes could be injured by widely separated lesions and the various cortical areas might be relatively "equipotential" in the mediation of higher intellectual association or synthesis.

There need now be little doubt that localized higher synthesizing or associating centers exist in the higher primates. Brickner (6)<sup>23</sup> has proven that in man the frontal lobes (prefrontal area) are such a center; injury to the frontal lobes produces a different clinical picture than that resulting from injury of equal extent to other parts of the cortex. The work of Jacobsen and his associates (45, 46) makes it now appear certain that both monkeys and chimpanzees suffer a particular kind of intellectual loss following destruction of or extensive injury to the frontal lobes. "Immediate memory" or "recall memory" as measured by loss of delayed reaction capacity and inability to respond appropriately to all the elements in a complicated tool problem are particularly vulnerable.<sup>24</sup>

Schilder's (93) work, which we have already mentioned, indicates very clearly that the parietal lobes also function as higher associative centers, and they mediate a particular type of intellectual process.

Extremely striking is the fact that these areas, the frontal and the parietal, were the areas traditionally conceived of as being associative areas. The existence of higher associative centers mediating specific types of intellectual processes is of course incompatible with theories of "equipotentiality" of cortical tissue, even as this "equipotentiality" applies to an integrative function.

These facts are not to be taken as meaning that all higher mental activities are localized in specific areas of the cortex, even areas as

<sup>23</sup> The data of Penfield and Evans (83, 84) support those of Brickner in spite of the brevity of their behavior data, and in spite of the fact that they place a somewhat different interpretation upon their results.

<sup>24</sup> As Jacobsen has pointed out, these data are very similar to those presented by Brickner for man. The present author doubts that all frontal lobe injury can be reduced to loss of "immediate memory" or even "synthesis."



extensive as the prefrontal.<sup>28</sup> Many complicated mental processes apparently have more widespread localization, as is indicated by human data. This problem is discussed in detail in the following section.

#### IV. THE HUMAN DATA BEARING ON THE NEURO-PHYSIOLOGICAL CORRELATES OF LEARNING AND INTELLIGENCE

##### *A. Introduction to the Material*

Throughout this review the author has emphasized the importance of the application of a principle of the existence of different kinds of function in attempting to correlate the facts concerning the neural organization with those of learning and intelligence. Credit for application of a principle of functional levels to the interpretation of data on the disruption of the higher mental processes is due Jackson (43), who insisted on a distinction between 2 levels of learned language responses: (1) an intellectual level characterized by propositionizing; and (2) an emotional level at which verbal responses were primarily indicative of emotional stress.

In the first section of this paper the case was presented for a rather arbitrary, four-fold categorization of kinds of learning:

Distinction was made between (1) non-voluntary conditioning, (2) skills, (3) perceptual learning, and (4) conceptual learning. Skills differ from the other kinds of learning in that they involve principally motor reorganization. Non-voluntary conditioning differs from perceptual and conceptual learning in that there is lack of conation and lack of insight into the nature of the new acquisition. Differences between perceptual and conceptual learning have already been given. Data from man indicate that beyond the level of "conceptual learning" or "symbolic formulation" there may be still more complicated mental processes and capacities that may be best expressed as "intelligence" or "special factors or abilities." Actually the capacities underlying learning of all forms must determine intelligence, at least in part.

Furthermore, if the author's interpretation of the data is correct, the degree to which learning and intelligence are localized within the cerebrum (and particularly the cerebral cortex) is largely a function of the complexity of the associative process. Thus, in general, the more complex the mental process, the more widespread are its neuro-physiological correlates; the simpler the mental process, the more

<sup>28</sup> In a personal communication, Jacobsen has suggested that further unpublished experiments indicate that "recall memory" is not equally affected by lesions in any part of the frontal area.

restricted and highly localized are its anatomical and physiological determinants.<sup>26</sup>

In considering the problem of localization certain principles must be kept in mind. Of foremost importance is the maxim of Hughlings Jackson that "To locate the damage which destroys speech and to locate speech are two different things." Likewise to locate the damage which may effect loss of some variety of learning and to localize that form of learning are different things. This is particularly true since most clinical cases are confused as to localization by many secondary factors, such as vascular disturbances, pressure, and "diaschisis," as has been emphasized by Monakow (78).

### *B. Localization of Primitive Speech Responses*<sup>27</sup>

One of the most striking facts in aphasia is the retention of low-grade or primitive speech responses almost regardless of the severity of the disorder. Concerning these language responses Head (34) has written:

"There are certain acts of speech which have little or nothing to do with thinking, and neither state a proposition nor culminate in action. These remain unaffected in aphasia and kindred disorders. They comprise meaningless words and phrases, emotional ejaculations, such as 'Oh! dear me,' together with oaths and other familiar expletives. I once had a patient under my care who could utter 'yes' but not 'no,' for all forms of negation or disapproval he employed the word 'damn.'"

Following Jackson and Head, Weisenburg and McBride (113) have pointed out the frequent sparing of low grade language responses, even after propositional expression is entirely gone. According to these authors, low grade speech responses may be divided into 3 groups: (1) the emotional, (2) "the automatic word series,"<sup>28</sup> and (3) the reactive.

<sup>26</sup> Such a hypothesis emphasizes the importance of "mass action" particularly for the higher types of organization. It does not, however, imply "equipotentiality" since the various centers acting may be discrete, carry out limited functions, and play entirely unequal rôles in the performance of different tasks.

<sup>27</sup> In the discussion of learned language responses the simplest verbal mechanisms will be considered under the title of primitive speech responses rather than the classification of non-voluntary conditioning. Language responses are probably never "pure" non-voluntary conditioned reflexes, since the speaking of words obviously involves skills. Certain of the "emotional" and "reactive" forms of speech closely resemble "non-voluntary conditioned reflexes."

<sup>28</sup> The present author believes that "automatic word series" may be best considered as simple verbal skills and reserves discussion of them for the latter part of this chapter.

Weisenburg and McBride write as follows:

"[The] 'reactive' forms of speaking [depend], for their appearance and their superiority to propositional statements . . . not so much on the fact that they have through years of experience become automatic, as on their issue as integral parts of the patient's reaction to some more or less pressing situation. Thus the patient may not only say 'Hello' or 'Good-bye' in appropriate circumstances, but he may bring forth a much more complicated propositional response than he could produce without the stimulus of the immediate situation."

Since these language responses are extremely simple, they should be more precisely localized than the more complicated language functions. Certainly no great cortical mass is necessary for their elicitation since they are often unaffected following widespread cortical lesions which may totally abolish propositional speech. Neither are the primitive language responses dependent upon the integrity of any of the higher associative or parareceptive areas, a fact that is indicated by their ability to survive widespread damage to the frontal, temporal, and parietal lobes.

Jackson attempted to account for the extreme persistence of these responses by suggesting that they might be mediated by either hemisphere. At least until very recently most neurologists have objected to this idea, as is indicated by the following quotation from Weisenburg and McBride:

"As Pick has well said, there is no clinical evidence to support Jackson's views on the part played by the right hemisphere. The more automatic processes in language and behavior differ from the less automatic in functional nature, but not in point of localization."

The observations of Zollinger (117) on the retention of certain inferior speech responses after complete removal of the dominant left cerebral hemisphere have since supplied the "clinical evidence to support Jackson's views" regarding the "more automatic processes in language." Zollinger found that his patient was able to say "thank you," "please," "good-bye," and "sleep" (meaning sleepy), within 5 days following complete resection of the dominant hemisphere. Apparently tests could not be conducted afterward and the patient died 17 days from the time of the operation.

It should be noted that the only language responses that were obtained were of the "emotional" or "reactive" forms of speech. This study, however, does not set the limits of the non-dominant hemisphere speech functions, since no patient can recover from such an operation within the few days during which tests could be made; and during every tested day increased speech capacity returned. Zollinger explains the retention of speech as follows: "Perhaps there has been a gradual development of a speech center in the right hemisphere

during the months prior to the removal of the left hemisphere." If such vicarious transfer were effected, it would indicate that the non-dominant hemisphere is natively endowed with language capacities.

### C. Localization of Verbal Skills

As has already been suggested skills differ from other forms of learning not only in complexity but also in nature, since skills involve "motor" reorganization to a greater extent than do most other forms of learning which are essentially "sensory" in organization.<sup>29</sup>

That such a difference exists has long been recognized by neurologists, and attempts such as Marie and Foix's (70) to combine motor and sensory aphasia into a single classification have not met with favor among neurologists, even the most "dynamic."

The verbal skills in all probability represent more than one level of complexity, and the various functional systems apparently operate in a hierarchical manner. Thus Zollinger's (117) work indicates quite clearly that very simple verbal formulations may be mediated by the motor centers lying either in the right hemisphere or, conceivably, in subcortical centers. The frequent sparing of "automatic word series" might also be taken to indicate a bilateral representation for this function.<sup>30</sup>

As might be expected the more complicated forms of motor expression are affected by the more diffuse lesions. Thus "syntactical aphasia," "jargonaphasia," and "agrammatism" are frequently

<sup>29</sup> Such a classification into "motor" and "sensory" learning is descriptive but not entirely accurate since the learning of skills obviously involves reorganizations of sensory systems. Thus apraxia and aphasia may both be caused by lesions in the temporal lobes or angular gyri. The following facts, however, attest to the separability of these forms of learning: (1) motor aphasia is usually caused by lesions anterior to the central sulcus, sensory aphasia by lesions posterior to the central sulcus; (2) apraxia is more frequently caused by anterior lesions, agnosia by posterior lesions; (3) amusia is sharply differentiated into expressive and receptive forms and this according to Feuchtwanger (18) has important bearing on classifications of aphasia; (4) "expressive" speech disorders, according to Weisenburg and McBride (113) come much closer to representing a unitary type of loss than do the "receptive"; (5) expressive disorders usually involve less intellectual deterioration than do receptive disorders; (6) serious disturbances in the mechanisms of speech production may occur without appreciable loss to the more complex mental processes, both verbal and non-verbal.

<sup>30</sup> True "automatic word series" were not observed in Zollinger's patient, but the fact that by the fifth day one compound word "goodbye" and one two-word phrase "thank you" had returned suggests that with further recovery simple "automatic word series" could have been formulated.

caused by lesions posterior to the central sulcus. Fairly widespread lesions, usually centering at or near Broca's area, are typically associated with "cortical motor aphasia." This disorder, according to such authorities as Head (34), Pick and Thiele (87), Goldstein (27), and Weisenburg (113) always involves some disorder of the symbolic function, as well as producing such typical motor symptoms as simplification of the language structure, poverty of words, and hesitation in speaking.

The simpler speech functions associated with sub-cortical motor aphasia, word-muteness, or anarthria are somewhat more precisely localized, being usually associated with sub-cortical lesions, or, according to Goldstein (27), with lesions in the general region of the operculum centrale.

#### *D. Localization of Perceptual Abilities*

Students of aphasia have long noted that lesions located within a single parareceptive (associative) area produce far greater loss in the learned responses and associations mediated by the sensory channel affected than in those associations mediated by any other sensory system. Thus lesions in the striate and periparastriate areas and the angular gyrus are typically associated with visual agnosia and word-blindness, whereas temporal lobe lesions usually produce auditory agnosia and word-deafness.

A vast number of studies have reported cases in which the injury was largely limited to a single receptive-associative system, and there can be little doubt that visual and auditory language mechanisms are differentially affected. Thus Henschen (35), on the basis of some 700 anatomical-pathological observations, concluded that the various visual and auditory language mechanisms can be sharply localized and that these 2 main receptive systems are discrete.<sup>31</sup> Piéron (88), in spite of certain "dynamic" leanings is also favorably disposed to the hypothesis that "pure" word-blindness and deafness exist. The recent, highly critical review and evaluation of this material by Weisenburg and McBride (113) indicates what have been the main sources of error in the past. Although the latter workers deny the

<sup>31</sup> With the possible exception of Kleist (50), Henschen stands as the leading modern advocate of strict localization. Those who advocate strict cortical localization usually favor the idea of the existence of "pure" word-blindness and "pure" word-deafness. Such theories of mental function are either agglutination theories, or modified agglutination theories postulating the existence of "higher association" centers.



existence of "pure" word-blindness and "pure" word-deafness, in the sense that these disorders can occur without any other sensory or intellectual involvements, they freely admit that either system may be more seriously affected than the other, depending on the site and nature of the lesion.

Actual examination of the data indicates that the most striking symptom in the relatively "pure" cases of word-blindness and word-deafness is that in one sensory system the capacity to carry out simple learning tasks is affected or destroyed, whereas in the other sensory system these same tasks are either spared or, at the most, slightly damaged. Thus these data clearly demonstrate that, for comparatively simple associations at the perceptual learning level, a considerable degree of functional localization exists; and that this functional localization<sup>32</sup> is specific to the appropriate sensory system.<sup>33</sup>

Recently German and Fox (25) have presented data which are relevant to the problem under consideration. Following recovery from an occipital lobectomy, the patient was given a battery of psychological tests by Sears and Snedden. As should be expected, tests involving visual associations were much more seriously affected than those involving learning of other sensory material. Thus, on tests of visual phonetic abilities, visual memory span, and visual-visual and visual-auditory associative learning, the patient's age score was *below* the 7 year level. On comparable tests in which the auditory system alone was involved (or possibly the auditory-kinesthetic system) the patient's age score was from 9 to 11.5 years. Thus the differences in ability on the tests were greater than from 2 to 4.5 years mental age; but the actual differences in ability were far greater than these test figures indicate. Other observational data support the same conclusions. Thus, when the patient was shown an orange and asked to name it, she was unable to do so. She was then allowed to smell the orange and immediately called it by name. In a second instance she was shown a bell and asked to name it. She was unable to do this until the bell was rung, when she named it immediately. Olfactory and auditory pathways were here sufficient to effect the

<sup>32</sup> Dusser de Barenne (16) has recently pointed out the desirability of the phrase functional localization instead of localization of function, in a recent short publication.

<sup>33</sup> Cases of visual agnosia and auditory agnosia indicate the degree to which auditory and visual material may be separated at the "perceptual level." Illustrative cases have already been given earlier in this paper. Only the data on aphasia will be considered at this time.

appropriate motor response after the visual association pathways had been interrupted.

Similar data are also presented by Fox and German (20) following temporal lobectomy.<sup>34</sup> Although the differences are less striking, since auditory perceptual learning was not as seriously disrupted here as was visual perceptual learning in the case above, the differences still exist. The investigators state, for example, that "auditory perception still (after 13 months) showed a defect in interpreting complicated verbal commands. This difficulty was absent when the commands were presented in written form." And again, "Retention of auditory material was definitely inferior to that of visual experience."

Special attention has been given to the 2 cases cited above, since in them the location of the lesions was accurately delimited and since psychological tests with a semblance of adequacy were reported. Cases of word-blindness and word-deafness reported by Piéron (88), and cases summarized and critically evaluated by Wiesenburg and McBride (113) substantiate the facts already presented. Indeed, in many cases the dissociation of the visual and auditory systems occurs to an ever more marked degree than in the cases cited; simple visual associations may be retained and auditory lost, or conversely, the auditory associations may be spared and the visual seriously affected.

#### *E. Localization of Conceptual Abilities or Capacities for Symbolic Formulation*

Two opposing neurological positions are held concerning the neuro-physiological correlates of that capacity for complex learning which we have designated as "conceptual learning." Advocates of the doctrine of strict localization usually maintain that conceptual learning is the product of the associations between sensory spheres. Advocates of a more "dynamic" point of view hold that a widespread cortical area is instrumental in the mediation of a more basic type of function, a function to which a number of different names have been given.

The former point of view is expressed by Schaller (92) in his statement of Henschen's contention: "Speech centers with sharp localization are the workshops of ideation (Begriffe). Ideation

<sup>34</sup> The two operations are not entirely comparable since occipital lobectomy destroyed the striate and periparietate areas of the dominant lobe, whereas the (partial) temporal lobectomy probably did not completely destroy the cortical auditory receptive and parareceptive areas.

depends on concepts (*Vorstellungen*) of different associated auditory, optic, tactile, etc., nature."

Head (34) has long been recognized as an advocate of the dynamic point of view. The following quotation indicates how seriously he disagrees with any explanation of higher mental processes in terms of sensory-sensory or sensory-motor associations:

"According to the older view of cerebral localization, various functions generated in different areas of the cortex are brought together like fragments of a mosaic to produce some higher form of activity. Should this be disorganized by a lesion of the brain, it was assumed that the elementary processes out of which it was composed must be revealed in their primary character. Thus the phenomena of aphasia were supposed to discover the motor, auditory and visual elements of normal speech.

"I have shown that such a conception is completely untenable and is not justified by the facts either of experiment or of clinical observation. No function is 'localized' strictly in any part of the cortex and no form of activity, somatic or psychical, is built up into a mosaic of elementary processes which become evident when it is disturbed by a lesion of the brain."

However, in considering Head's interpretations it must always be remembered that the level of integration in which Head is interested is that of "symbolic formulation." Furthermore it should be noted that even at this level Head not only admits, but even insists on, functional localization, in that he claims symbolic formulations to be the product of activity of the dominant hemisphere and each of his 4 forms of symbolic formulation to have their foci of activity in somewhat different cortical areas.

Many modern investigators have recognized a level of mental capacity analogous to the level of "conceptual learning" suggested in this paper, and most clinicians have reported loss of this capacity resulting from lesions located in widely different cortical areas. As to the exact mechanism underlying this level of function there is considerable disagreement.

Data from occipital and temporal lobectomies are of particular value as critical tests of a theoretical position if they include *adequate psychological tests*,<sup>35</sup> since either the occipital or temporal lobe consists of a single receptive-parareceptive system. Fox and German (20, 25) have obtained data on the effect of temporal and occipital lobectomies which are of especial value. These investigators have interpreted their

<sup>35</sup> Penfield and Evans (83, 84) have reported the effect of lobectomies, but, as they admit, the behavior data are often scanty and inadequate. Reference to their results will be made later.

studies to favor a theory of functional localization which would accord with Henschen's theory, for they say:

"Determined by its functional components: perception, memory, thought, the flow of movements and of skilled acts, the intellectual defect appeared to bear a relationship to cerebral localization and dominance, rather than to cerebral mass (Lashley)."

Despite the above interpretation of their own findings, actual examination of their data seems to show that loss of "conceptual learning" or "symbolic formulation" always extends beyond the sensory sphere affected. Indeed, the complicated associations based on the sensory systems left intact appear to be almost, if not as, severely affected as those based on the system damaged.

For example, in the case of left temporal lobectomy they reported that auditory *perception* was more seriously affected than visual *perception*, and auditory retention was more seriously affected than visual retention; while at a higher conceptual level serious loss is reported for both sensory modalities. This is apparent from the following quotation in which Fox and German describe the condition of the patient:

"When he (the patient) was listening to speeches on the radio he could understand everything that was said during the first few minutes. After that the words seemed to become 'jumbled' and all continuity was lost. *Essentially, the same difficulty was experienced when he was reading. He would progress fairly satisfactorily for the first few paragraphs.* Then he would begin to miss the meaning of some words, and the mistakes would multiply rapidly. Finally, as the patient explained it, his 'mind became so full of words' that *he lost all significance of what he was reading and was compelled to stop.*" (Present author's italics.) The fact that the patient's score on performance intelligence tests is somewhat below his expected I.Q. as indicated by his educational status is also indicative of loss of higher mental capacities. A similar conclusion can be drawn from the fact that his "mathematical ability was about 25 per cent below the average."

Another case reported by these same authors is one of resection of the anterior one-third of the temporal lobe. Here again there was loss of conceptual and intellectual components of language which was not limited to auditory material (as indicated by paraphasia, difficulty in naming series of objects, and difficulty in giving definitions and full sentences).

In the single case of occipital lobectomy reported by German and Fox (25), there is no doubt that *simple* visual perceptions and associations were much more gravely affected than *simple* auditory perceptions and associations. Yet the loss of more complicated language

functions was not restricted to visual material—a fact which was indicated by defective ability to correlate word memories, limited vocabulary, definite impairment of the intellectual components of speech, slowing of mental processes, lack of “will” to work rapidly, and by the fact that Sears and Snedden (who carried out the psychological studies) reported that on the Stanford-Binet “those items involving little or no visual activity appeared to be no better than those definitely visual in nature.”

It can, of course, be argued that the loss of the higher mental functions involving the unaffected sensory sphere is brought about indirectly by injury to the affected sensory sphere. This can neither be absolutely proved nor disproved. Yet it would appear to be very difficult to account for non-language as well as language disorders, amnesic disturbances, and disorders of Head’s semantic type on the bases of strict associationist doctrine.

From the data presented it is obviously difficult, if not impossible, to localize the center or centers which mediate “conceptual learning.” So far as language symbolism is concerned there can be little doubt that the dominant hemisphere is of primary importance. The little available evidence suggests that the cortical receptive areas are less essential in the mediation of these functions than are other cortical areas. Concerning the localization of semantic and nominal aphasia Head (34) suggested that:

“Both forms of defective use of language can be evoked most readily by injuries which fall over an area lying between the post-central and interparietal fissures, that is, over the supra-marginal and angular gyri and the parts beneath them.”

Yet this probably does not delimit the areas which may be involved in defects of the conceptual kind. The work of German and Fox suggests that the visual and auditory parareceptive areas probably play some part in these mental processes. (It may appear that these arguments imply that all these cortical areas are homogeneous or “equipotential.” This is not necessarily true since the capacity for conceptual learning or symbolic formulation may represent a number of different abilities—this in spite of the fact that attempts to make such analyses have met with little success—and the different abilities may be unequally represented in the various cortical centers.)

#### *F. Localization of Intelligence*

Loss of the capacities which underlie the various forms of learning already outlined must of necessity involve some intellectual deteriora-



tion.<sup>36</sup> Yet clinical observations and data from verbal and non-verbal intelligence tests suggest that widespread cortical lesions may produce even more complicated types of mental disturbance than those already discussed.

Since a wide variety of disorders fall into this grouping, an adequate classification is difficult. The material, however, will be discussed under the following headings: (1) ability to maintain and hold an attentive attitude, (2) memory, (3) rate of work, (4) mathematical ability, and (5) non-verbal intelligence.

(1) Loss of the ability to maintain attention, loss of initiative and inhibition, and euphoria have frequently been reported in cases of aphasia and also following resection of large masses of cortical tissue from widely separated cortical areas. All of these suggest some "release" of higher inhibitory control. Such a concept has been stressed by Head (34) as loss of "vigilance," and by Piéron (88) as lowered "cortical metabolism." As one might expect, disorders of this type are particularly likely to appear after lobectomies, since these involve destruction of a large cortical mass.

Penfield and Evans (84) report that a right frontal lobectomy resulted in "a lack of capacity for planned administration," a left frontal lobectomy left the patient with "fatiguability" and "some lack of initiative," and that another patient following right temporal lobectomy was "mentally . . . quite normal although slow in his reactions and not a match for other members of the family." Following left (dominant) temporal lobectomy Fox and German (20) noted that an intelligent subject "did not chafe at the long unemployment which his illness necessitated" and that there was some "emotional excitability." The same authors, in a later communication, stated that, following left (non-dominant) frontal lobectomy, a patient showed signs of "definite euphoria" and "loss of initiative." Partial removal of the dominant temporal lobe induced "marked euphoria" in one patient, and left (dominant) occipital lobectomy in another patient resulted in a "tendency to tire easily and slight euphoria." Following bilateral frontal lobectomy Brickner (6) observed that "Chief among the usual symptoms of frontal lobe injury which A shows, are inattentiveness, distractibility . . . and lack of initiative, or apathy."

The truly surprising fact is the frequency with which these symptoms appear whenever psychological data are reported. Penfield and Evans make the significant statement regarding one of their

<sup>36</sup> Since this is apparently true there is justification in the assertion of Marie and Foix (70) and Moutier (79) that aphasia represents a defect of general intelligence as well as a special language defect. Although this is denied by Weisenburg and McBride (113), these authors cite no case in which an aphasia patient ever attained a level of performance equal to his educational level, if all non-language tests are included.

cases: "He had lost something which psychometric examination does not evaluate. He has lost initiativeness: not all of it, but much of it."

(2) Memory defects are frequently found following cortical lesions. Memory, of course, is no mental faculty *per se*, but exists solely as the capacity to reproduce specific acts or experiences.

The most striking defect of human memory recorded in aphasic disorders is that occurring in amnesic aphasia. This syndrome has been noticed by most modern workers, including Henschen (35), Goldstein (27), Pick and Thiele (87), and Weisenburg (113), and is characterized by the difficulty or inability to produce words as names, independent of other language disturbances. From the psychological standpoint the most striking characteristic of amnesic aphasia is the *retention of recognition memory coupled with the almost complete loss of recall memory*. This is clearly expressed by Weisenburg and McBride (113) who write:

"... One of the most outstanding characteristics of the clear-cut amnesic aphasia, [is] the ability to recognize the correct word. Whether he produces it himself or hears it suggested by the examiner, the patient almost always identifies it, repeats it with some satisfaction, but shortly loses it again. The percentage of correct recognitions is far higher for the amnesic patient than it is for the receptive patient who has difficulty in word finding.

"It [amnesic aphasia] consists essentially of a difficulty in evoking [recalling] words as names for objects, conditions, or qualities. . . ."

As might be expected, amnesic aphasia is typically associated with widespread lesions.<sup>87</sup> Wilson and Winkelman (115) have recently suggested that widespread destruction of the third and fourth layers of the cortex may be frequently associated with memory loss of one type or another.

(3) Decrease in rate of work may be induced by widely differing cortical lesions and has long been recognized as a symptom of aphasia. This symptom has been critically discussed by Weisenburg and McBride (113) as follows:

"Slight aphasic disorders may first appear as a retardation in certain language performances, and slowness in speaking and other performances may be the last clear sign of difficulty as a more severe disorder improves. Further-

<sup>87</sup> As a result of experiments on monkeys and chimpanzees, Jacobsen and his associates (45, 46) have associated loss of recall memory with the frontal lobes. Brickner (6) has also pointed out that recent memory is seriously affected by bilateral frontal lobectomy, although he holds that the underlying mental defect is a loss of "synthesis." Bilateral frontal lobectomy does not cause amnesic aphasia.

more, the majority of aphasic patients are retarded in *both verbal and non-verbal responses* [present author's italics], some of them requiring five to ten or more times as long as the normal to complete a given task. From the point of view of rate of work, however, there is no rule for aphasia in general, nor are there absolute divisions according to types of disorder."

Marked slowing of response to both language and non-language tests was reported by Fox and German (20, 25) following removal of the dominant occipital lobe and also following removal of the dominant temporal lobe. Slowness of response was also reported by Penfield and Evans (83) as a result of temporal lobectomy.

Weisenburg and McBride (113) present some data suggesting that responses are slowed following lesions in the non-dominant hemisphere. On the basis of 5 tests, one a verbal test, the others performance tests, in which a comparison on the basis of speed might be obtained, they reported that:

"On this one language test the cerebral cases are equally poor in comparison with the matched normals in speed and in quality scores, . . . on three of the four 'performance' tests they are further below in rate than in quality of work."

(4) Some loss of mathematical ability is usually associated with all forms of aphasia. Thus, Weisenburg and McBride have noted that some impairment of mathematical capacity usually occurs in receptive, expressive, and receptive-expressive aphasia. Likewise Head has commented on the fact that:

". . . It would be impossible by examining the answers to simple problems in addition or subtraction to determine the variety of aphasia to which the patient belonged. The harder the intellectual task the more likely is he to fail in solving it correctly; but there are not those profound differences according to the form assumed by the defects of speech, which are apparent in the acts of counting or recognition of numerical meaning."

Severe loss of mathematical ability has been reported following bilateral frontal lobectomy (Brickner, 6), and left temporal lobectomy (Fox and German, 20). Special loss of mathematical ability has been localized in the angular gyrus as a result of "cipher blindness" (Henschen, 35) and as a symptom associated with finger agnosia (Gerstmann, 26).

Mathematical ability would obviously seem to depend on the close interaction of many complicated mental functions and the loss of this ability following disparately located lesions might be explained as the loss of the special function of each of the centers involved. This may be partially true, but it fails to explain why loss of mathematical capacity should appear following

injury to the non-dominant hemisphere. The degree to which this loss occurs is indicated by the following quotation from Weisenburg and McBride (113):

" . . . It seems clear that cases of right-sided lesion without aphasia do not manifest a change similar to the aphasic. Instead of differing from the normal most clearly in language performances as the aphasic patients do, they show greater differences in arithmetic and non-language work. They seem to be not only inferior to the normal group, to an extent which is impossible to predict accurately from the present sample, but qualitatively different."

(5) Loss of non-verbal intelligence, as Weisenburg and McBride have pointed out, is a frequent occurrence in aphasia. Furthermore, "Most cases of aphasia show a certain amount of deterioration in non-language performances beyond that which could be explained on the basis of decreased speed of work." And again, ". . . the non-language response shows (sometimes) a disturbance which cannot be explained on the basis of the demonstrable language changes." In rare cases there may be "extensive language deterioration with little or no change in non-language performances."<sup>38</sup>

On the basis of comparison made between 22 cases of right-sided cortical lesions without aphasia and a group of "matched normals" Weisenburg and McBride (113) conclude that cases of right-sided lesion without aphasia, "instead of differing from the normal most clearly in language performances as the aphasia patients do . . . show greater differences in . . . non-language work."

From the data presented there is no reason to believe that lesions in the non-dominant hemisphere produce any more serious loss of non-language performance than do lesions of the dominant hemisphere. Furthermore, inspection of their Figures 15 and 17, in which Weisenburg and McBride summarize the results obtained on cases of lesions in the non-dominant hemisphere, reveals that only 2 non-language tests, the Porteus maze test and the substitution test, showed serious loss. At least 1 language test, the sentence completion test, was as seriously or more seriously affected. Comparison of the cerebral lesion and matched normal group indicates that the Pintner-Toops reading test was more seriously affected than the Pintner non-language test. Thus there is some indication that the tests of greatest complexity, the tests demanding "constructiveness" to the highest degree, whether language or non-language, are the tests which show greatest loss. These data combined with the data already dis-

<sup>38</sup> Unfortunately only little use of non-language tests has been made by other investigators. Fox and German's reported cases of occipital and of temporal lobectomy in the dominant hemisphere were followed by loss on performance tests.

cussed on the problem of intelligence might be taken to indicate that the higher intellectual functions are diffusely represented throughout both hemispheres, but that the dominant hemisphere has, in addition, preëminent control of language.

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## SPECIAL REVIEW

### MURCHISON'S HANDBOOK OF SOCIAL PSYCHOLOGY \*

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This large volume consists of twenty-three essays on various social topics, a short preface, and two excellent indexes. Every article contains a list of references, and a number of them are illustrated. Some of them are in other fields than social psychology. In attempting to differentiate them I shall not rely upon my own definitions, but upon the subject-matter and categories that are found in the books and monographs of investigators in the various fields. As social psychology I shall include everything that social psychologists in general treat as social psychology. My own rough classification of the articles would assign nine to aggregations and social relations among plants and animals, six to sociology and cultural anthropology, one to language, one to ontogenesis with reference to social psychology, one to sex differences and certain relations between sexes, three to concepts and hypotheses, and two to experimental social psychology. With this wide range of subject-matter there is wide difference in point of view and specificity of problem, method, fact, and theory. I know of no other way to deal with this heterogeneous material than to write twenty-three reviews of as many separate and unrelated articles. When this is done I shall be in a position to add some general comments on the book as a whole.

The editor has classified the essays into five groups. The first, entitled "Social Phenomena in Selected Populations," comprises *Population Behavior of Bacteria* by R. E. Buchanan (17 Pp., 36 Refs.), *Social Origins and Processes Among Plants* by F. E. Clements (27 Pp., 32 Refs.), and *Human Populations* by W. S. Thompson (31 Pp., 54 Refs.). Although the first of these articles is sheer biology, and the third straight sociology, they are in many respects similar. Both are searching for the quantitative laws of growth in their respective populations; both deal with accretion as determined by the biological laws of reproduction and the physical and chemical

\* Murchison, C. (Ed.), *Handbook of Social Psychology*. Worcester, Mass.: Clark University Press, 1935. Pp. xii+1195.

environment. The result is the production of aggregates whose populations wax and wane as both sets of conditions vary. Thompson, of course, also discusses the effects of pestilence, famine, and the social conditions that in part control the rate of growth of human populations, *e.g.* war, infanticide, emigration and immigration, and birth control. His interest is sociological and he does not speculate on problems that belong properly to social and individual psychology. Buchanan reveals a similar restraint. Among bacteria there is a difference in group configuration depending upon physical and chemical conditions which, by way of modification in the shape, size, and appearance of single cells, changes the form of the group—rods, chains or colonies. Furthermore, bacteria show symbiosis (two organisms are mutually beneficial to each other), anabiosis (the one utilizes the waste products of the other), and antibiosis (one, by the production of deleterious products, antagonizes the other). The author is a biologist and he draws no analogies to human social psychology. Clements, however, takes the opposite tack. He deals with plant ecology in terms of families, clans, colonies, and communities, of social bonds, of coöperation, reaction and coaction, of disoperation and competition. The title of his article together with occasional passages in it suggests that he regards the same social functions and their end products (the various social groupings) as common alike to plant and animal life. It is not for a psychologist to say how useful this notion may be for plant ecology. But, for social psychology it is dangerous; for, to take a single instance, the difference between two plants competing for the chemicals of the soil and two men competing for the approval of their fellows is so great that the analogy is misleading.

The second group of essays—"Social Phenomena in Infrahuman Societies"—consists of *Insect Societies* by O. E. Plath (59 Pp., 119 Refs., 22 Figs.), *Bird Societies* by H. Friedmann (43 Pp., 69 Refs., 7 Plates), and *The Behavior of Mammalian Herds and Packs* by F. Alverdes (19 Pp., 19 Refs.). The first is strongly reminiscent of an older generation. The article deals with the sub-social insects, social wasps, social bees, ants, termites and their guests and parasites in great detail but in the romantic manner of the old time naturalist with all the imagery of human societies—kings and queens, royal chambers, soldiers and organized warfare, slaves, workers, domestication. If a proper description requires a pictorial presentation, why not take a modern figure and deal with the social insects as factories organized for the mass-production and mainte-

nance of a particular species? Emphasis would then be placed on the reproductive and hunger instincts, and perhaps the social entomologists could discover more about the mechanisms by means of which these instincts become effective. The freshest article of this group is that of Friedmann on birds. He distinguishes between mere gregariousness (which results in aggregations) and social organization. In bird migrations some species, like the Canada geese, have flock formations, and in many of the small passerine birds the large groups are composed of families from limited areas of the breeding range; often also there are sex aggregations, one flock consisting of males, another of females. Otherwise there is little evidence of social organization among birds. There are, however, many different forms of aggregates. These depend upon breeding and feeding habits and are classified as breeding groups and feeding groups. Many examples of each are described. The topic of communal nesting is illustrated with several excellent photographs. His point of view is sociological rather than psychological. He does not consider the mating pair as a group and hence speaks of solitary nesting; he mentions only in passing such social performances as 'pecking' dominance, territorialism, sentinels (among rooks), competition (among cormorants), etc., all of which are as significant for social psychology as group organization. The article is, nevertheless, a useful one.

The third essay of this group is distressing. Alverdes begins with a running account of the ways in which mammals mate, monogamous or polygamous, solitary or in herds, seasonal or permanent. Then the family is discussed under three heads: parental, paternal, and maternal. This is followed by a description of those animals that run in herds and packs, their sexual groupings, groups with leaders, groups of different species. The article ends with cases of mutual help, cannibalism, courtship and mating, play, imitation, communication, etc. The article belongs to the post-Darwinian period. Many of the author's statements are questionable; some are flatly contradicted by other authors. No sources are mentioned except German; there is no evidence of any knowledge of the experimental psychology of animals.

With the next group there is a sudden change in scene. Under the title, "Historical Sequences of Human Social Phenomena," there are 250 pages of cultural histories. The first, *Social History of the Negro* by M. J. Herskovits (61 Pp., 138 Refs., 5 Plates), is in form and treatment a model of what a summary article in cultural anthropology should be. The distribution of the negro race, the various

types of negroid peoples, migrations within and contacts without Africa, the cultural areas of Africa and their characteristics, African institutions and the psychology of the African, how the negro came to the new world, and the give and take between the cultures of the negro and those with which he came in contact—all of these topics are discussed neatly, succinctly and interestingly. By the psychology of the negro he means an account of those traits which either are prized by the negro or are revealed in his religion, his laws, and other institutions.

Clark Wissler follows with the *Social History of the Red Man* (41 Pp., 41 Refs., 1 Fig.). He writes hurriedly and not at a handbook level. But by a clever device he reduces a large geographical area and a number of different cultures to manageable proportions. He indicates and briefly describes three main types of aboriginal culture as of the year 1492. These are the city states and empire builders (Incas and Aztecs), the agricultural, and the hunting tribes. Then follows an account of the general aboriginal characteristics and an historical perspective. Aside from a few pages on the 'mental life' of the people—their use of magic, their religious beliefs, their tolerance, their inventions, and their sense of dignity—there is little having a direct bearing on social psychology.

The next two articles in this group are the *Social History of the White Man* by W. D. Wallis (52 Pp., 24 Refs.), and the *Social History of the Yellow Man* by E. D. Harvey (54 Pp., 26 Refs.). Since there are so many cultures of both races widely separated in space and time, a comparative study of the various cultures within a single race would seem to be the only way in which an adequate history could be written. Even then it is by no means certain that a single cultural history would emerge. It is with an especial interest, therefore, that we turn to these two essays to see how the problems in exposition were solved. Wallis begins with an excellent sketch of the physical characteristics, the probable place of origin, the migrations of the white race, and types of its earlier cultural achievement including those of Greece and Rome. He then leaps to the modern world beginning with the Renaissance. Here his style becomes highly rhetorical and it thus continues, with occasional lapses to a less dispassionate treatment, through a panegyric on science and technology, and literature and learning. The fourth section is a general article on the rôle of cultural contacts and diffusions. The social history of the white man has disappeared. There is a page on the diffusion of the radio, and there are nine pages on the

growth, spread, and local diversification of the "Christmas complex" (the Christmas festival). Nothing is said of the significance of these things in the social history of the white man. The article ends with one section on recent social trends, and another on efforts to understand, control, and direct social life.

Harvey in his account of the social history of the yellow man considers only those mongoloid cultures that have made some contribution to "the stream of world progress." These are the Chinese, Korean, Mongol, and Japanese. The author begins with the early social history of the Chinese as reflected in the stories of legendary kings, and continues with the early political organization as represented by the Chow constitution. He then discusses the influence of ancestor-worship on the structure and functions of the family, and of Buddhism on the life of the people and on the art and philosophy of the upper classes, and of a social system so bound with custom and so wanting in outlook that progress is hardly possible. Then follows an interval of perhaps a thousand years concerning which the author has nothing to say; instead he turns to the social awakening of the present century. The cultural history of the Koreans is a short story of the influence of Chinese thought on its early culture, of the unhappy lot of the lower classes, and of the reforms since 1904 under Japanese direction. The history of the Mongols is for the most part a brief report of the conquests of Genghiz and his followers in the 12th and 13th centuries. After this irruption the Mongols returned to the stage of culture from which they had emerged, a stage which the author compares to that of the Navajo American. The history of the Japanese is, thanks perhaps to Lafcadio Hearn, the best cultural history of this group. First is described their early clan organization, their weapons, utensils, food, etc., then the feudal organization which lasted for a thousand years, then the infiltrations of western cultures. The author reveals a warm sympathy for the yellow men and predicts new stages in their cultures and new contributions from them to human welfare at large. Neither he nor Wallis opens vistas for social psychology.

Part IV of the *Handbook*, entitled "Analyses of Recurring Patterns in Social Phenomena," consists of three quite unrelated papers. The first is on *Language* by E. A. Esper (44 Pp., 87 Refs.). It is not a comprehensive study of the social psychology of language. Its aim is said to be a "practical" one, the presentation of "view points and data which might suggest research in the field of



language of an experimental or other controlled and objective kind." The author's method is to paraphrase and sometimes to discuss some of the views of a selected group of authors. He rejects most of the work of those students of language who are tainted with "mentalism," who regard the social function of language as the communication of feelings and ideas, and he accepts those views which are objective or behavioristic. By restricting language to oral stimulation and response, and the growth of language to its acquisition by the child, it obviously may be studied objectively and, as the author demands, for its own sake. But the student of social psychology who is interested in language either as a means or as a product of socialization will find little in the essay to aid him.

The second article in this group is *Magic and Cognate Phenomena; an Hypothesis* by R. R. Willoughby (59 Pp., 66 Refs.). The author describes briefly every one of a long series of phenomena; fantasy, hypnagogic imagery, pseudo-hallucination, auto-hypnotism, drug effects, traumatic experience, crystal-gazing, divination, superstition, mediumship, dream, hallucination, apparition, omnipotence of thought, omen, portent, phobia, tabu, verbal magic, myth, magic (rite), religion (rite), obsession, sorcery, witchcraft, psychotherapy, art, construction, and science. The hypothesis is that these phenomena constitute a more or less continuous ascending series "of defensive efforts of the organism to neutralize or resolve tension." Within the series the defensive reactions are grouped according to the manner and degree of the neutralization or resolution. There is also a fifteen-page section on the social significance of anxiety control. Any hypothesis that relates a large body of hitherto unrelated phenomena is intriguing, but in the present instance the hypothesis is not worked out in sufficient detail to make it reasonable. Too frequently the author merely describes a phenomenon without showing how it fits into the hypothesis. Some of the "reactions" like tabu and black magic may be as effective in creating as in relieving tension. Finally the wonder is what it all has to do with social psychology. The author occasionally speaks of "group-tension," but uncritically. The tensions themselves are generally individual, the social environment is an important but not always a necessary condition for their arousal, and the reactions are individual.

Clark Wissler follows with an article on *Material Culture* (45 Pp., 55 Refs.). It is a clearly written, elementary, and summary account of the "material possessions of man, savage and

civilized," and the part they play in his culture. There is first a discussion of the use that cultural anthropology and archeology make of man's material possessions in understanding or reconstructing a particular culture or the history of culture. Then follow brief accounts of a few of these possessions, and discussion of topics related to them, *e.g.* fire, food, domestication and herding, agriculture, housing, transportation, textiles, cordage, weaving, basketry and matting, cloth, textile designs, clothing, ceramics, metals, tools, the graphic arts, primitive economics, work, and organized labor. At the last there are comments on the relation of material culture to culture at large and to its history. The article as a whole is useful, but is cultural anthropology, not social psychology.

Part V contains five articles classified as "Analyses of Some Correlates of Social Phenomena." The first of these is *The Physical Environment* by V. E. Shelford (29 Pp., 23 Refs., 13 Figs.). He conceives of a social community consisting of certain plants, certain animals and men all of which are adapted to the same or similar physical conditions, *i.e.* to a certain average rainfall, a certain average temperature, and a certain average humidity, all within certain limits. A deciduous forest is a community of this kind, and it is the one to which the white man of western Europe early became habituated. When Europeans emigrated to America there was little change in their physical environment in so far as averages and extremes are concerned. There are, however, annual, seasonal, and often diurnal variations in the physical environment; in particular there are extremes in heat, cold, rainfall, and humidity. The denizens of the community are affected by these variations in various ways either directly or indirectly, and it is the author's problem to determine these effects. Man, by virtue of his material culture—houses, fires, clothing, food-supply, remedies against disease, etc.—has become increasingly able to protect himself in large measure against the variations. This, however, complicates the problem. Furthermore, it is much easier to experiment with animals under these variable conditions than with man; consequently it is proposed first to determine the effects of the physical environment upon other animals, and then to infer from them the effects upon man. The author objects to the solution of the problem by the laboratory method in which all the variables are under control and their effects studied separately; for that is not the way in which animals and man live. His results therefore must be of the most general kind. He can never be certain that the effects charged to

excessive rainfall, for example, may not be in fact due to a variety of other conditions. The reader who struggles with the argument (which is not clearly presented) will be left with a multitude of queries.

The second "correlate" is *Age and Human Society* by W. R. Miles (87 Pp., 131 Refs.). The first section, entitled "The Continuity of Man," deals in a semi-literary way with such topics as the immortality theme, longevity, age and population structure, and individual *vs.* race continuity. In a second section—"Changes in Human Function with Age"—the author marshals the results of many researches on the anatomical, physiological, and psychological changes with age. It is at once the most authoritative, the most interesting, and the longest part (more than a third) of the chapter. Thus far the author has dealt with the individual. In a section on "Age and Group Relations," he turns to social psychology. Following Dodge he characterizes the social relations of man as "mental contact," and he writes briefly on "Age and Radius of Human Conduct," "Old Age and the Primitive Group" (a topic on which the author does not do full justice to the literature), "Age and the Family Group," "Age and the Community," and "Social Contact with Older People." There are few data in most of these topics. The comments are interesting but as they stand they beckon for further investigation. The chapter ends with a section on "Human Age and Industry"—a contribution to sociology with frequent psychological implications.

The third "correlate" is *Sex in Social Psychology* by C. C. Miles (115 Pp., 368 Refs.). After a brief introduction on the science of sex, the author turns to her main topic, a sixty-two page, competent analysis and cautious summation of the vast literature of sex-differences. The next section (marked C when it should be III) turns to sex behavior in the "individual social life," and treats the various ages from infancy to senescence. Premarital adjustment, marriage, parental functions, family and social adjustments are dealt with as sub-topics of "Sex Behavior in Early Maturity." The final section is devoted to sex in the community life, a sociological rather than a psychological account of sex in education, health, industry, and occupation, and the creative arts. The article is no less useful for its classification of the extensive literature that centers upon sex differences than for its excellent summaries. It is a good example of the typical handbook article but is not altogether happy in a handbook of social psychology.

The fourth "correlate" is *Attitudes* by G. W. Allport (47 Pp., 139 Refs.). It is a critical examination of the concept of the attitude, its need in psychology, its history, its definition, its genesis, the question of its "motive power," its kinds—positive and negative, specific and general, public and private, common and individual—its measurement, and classification of its kinds. Those psychologists who have considered the attempts of certain sociologists to psychologize the social attitude will welcome this study by a psychologist who is acquainted with its history. The result is a warning although the author does not thus intend it. He believes that the concept is necessary; he clearly perceives many of its difficulties and dangers. These he faces honestly, examines tolerantly, sets forth clearly, and then accepts. Perhaps nothing constructive can at the present time be done. Perhaps we must wait until the concept has become an entity (as it will become), and until it is evoked as *the* one and only explanatory principle (as it will be). Then it may be realized that the attitude is either a hypothetical concept, an explanatory term, whose seat is probably some condition of the nervous system, but of which we know next to nothing, or else it is a purely descriptive term for certain products of psychological functions. The cautious student of social psychology will, however, consider the article one of the most timely and useful of those in the *Handbook*.

The fifth and last "correlate" is *Social Maladjustments: Adaptive Regression* by F. L. Wells (71 Pp., 307 Refs., 3 Figs.). The writer undertakes the founding of a generalizing principle which he calls adaptive regression. By this term he means that kind of regression in which the individual, or the group, regresses from a higher, more complex, or more active function to a lower, simpler, or less active one. It has a genetic aspect—a regression to earlier behavior patterns; a dynamic aspect—a regression to an easier way; and a social-ethical aspect—a shirking of one's duties. It is this concept then which the author extends to include many phenomena common to mental disease, to normal recreation and rest, to social and cultural changes. He brings it into relation with sublimation, infantile factors and symbolisms, personality adjustment, dissociation, regression in the psychoses, and maladjustment in antisocial conduct. No one can read the article without appreciating the author's erudition, his skill in the manipulation of his materials, his grasp of a highly technical and semi-philosophical literature. Then, however, the reader may wonder whither adaptive regression

leads. It is no doubt an achievement to bring some of the phenomena of the psychoses and neuroses into relation with animal hypnosis, a summer's vacation, and "passing the buck"; but, what then? Perhaps the answer must wait until this highly speculative topic regresses—or sublimates—into the language of normal human psychology.

Part VI, the last section of the *Handbook*, is entitled "Experimental Constructions of Social Phenomena." The first of the papers under this heading is *Relative Simple Animal Aggregations* by W. C. Allee (28 Pp., 33 Refs., 8 Figs.). It is a companion to Clements' and Shelford's articles. But there is a marked difference in point of view, in the experimental basis of the conclusions, and in treatment. Although the author believes that there is a "very general and apparently highly fundamental tendency of organisms toward inter-individual coöperation," and although he finds among the lower animals the earliest manifestation of this tendency which in man reaches its highest development, he avoids the analogies which put the results of Clements and Shelford in a false perspective. In addition to a study of the factors which condition aggregations, he discusses the effects of aggregations, *i.e.* the physical and chemical changes in the environment, the effects on growth, on rate of production, on longevity, on behavior, the morphological effects of crowding, the effects of aggregations on sex, and finally, the social significance of aggregations. His interest naturally is biological, but the essay is an excellent contribution not only to the social psychology of the lower animals but also to that of all social groups.

The next article is *Social Behavior of Birds* by T. Schjelderup-Ebbe (26 Pp., 66 Refs., 2 Figs.). In anthropomorphic language, although anthropomorphism is disclaimed, the author describes in great detail the hierarchy of dominance (his term is despotism) among birds. If a pair, one bird dominates the other; if more than a pair, C may dominate D but may be dominated by B and the latter by A. A pure rank order, however, need not exist; D may be dominated by A and also by C, but D may dominate B who dominates C. There are other irregularities. The author discusses the ways in which "despotism" is expressed, its dependence upon age, size, sex, the season, sickness, acquaintance, sympathy and antipathy, social position, intensity of illumination, and weariness. The article is interesting and informative but not of the handbook type. There are no citations to other investigators, no experiments



are described, nothing is said of method beyond the statement that the observation of these phenomena requires the ability to recognize every bird of a group individually, and some of the references have no bearing upon the subject-matter of the essay.

The following article is *Social Behavior in Infrahuman Primates* by R. M. Yerkes and A. W. Yerkes (61 Pp., 36 Refs., 13 Figs.). It is a study primarily of problems and methods. The authors chose this procedure partly because much of the extensive literature of the subject is fragmentary and unreliable, partly because they think it is more useful. In the interest of concreteness, they limit their discussion to the lemur, the howling monkey, the baboon, and the chimpanzee, *i.e.* to a single representative species of each of the four orders. Nevertheless, despite the emphasis upon problem, they manage to give all the information that they think is reliable about the social behavior of the species they discuss. Particular problems center upon such topics as play and ownership, communication, affective behavior, social facilitation (a term substituted for imitation), parent-child relations, nostalgia in the primates, dominance-submission, and a number of topics grouped under the head of social service such as defending, guarding, caring for the sick and injured, grooming, etc. The article ends with a brief discussion of primate sociology and the socially significant differences between monkeys, apes and man. Notwithstanding a looseness of structure and a lectural style, it bears the stamp of authority. Its frankness, its caution, and its emphasis on further research make it no less useful for the social psychologist at large than for the special investigator.

The two remaining articles are *The Influence of Social Situations upon the Behavior of Children* by L. B. Murphy and G. Murphy (63 Pp., 64 Refs.), and *Experimental Studies of the Influence of Social Situations on the Behavior of Individual Human Adults* by J. F. Dashiell (62 Pp., 97 Refs., 4 Figs.). These two discussions represent the statistical approach to certain social problems by way of the stimulus-response logic. The Murphys devote the greater part of their space to theoretical problems, a procedure to which they are apparently driven by a mass of statistical results that resist generalization. In a first section they point out two sources of error: an "organism-error" which consists in regarding behavior traits as fixed attributes of organisms, as if the organism possessed certain traits without reference to stimulus situations; and a "situation-error" which consists in the presumption

that the organism tends to respond in a certain way when stimulated by a situation of a particular kind. This leads first to a discussion of variability and consistency of behavior, then to an examination of the atomistic and functional approaches to the problem of social behavior. A second section deals with methods of collecting and analyzing data in this age period. A third is given to a summary of experimental findings. The most significant part of the paper is its theoretical discussion. The authors attack their problems with courage and sincerity, and their thinking deserves consideration and criticism of a kind beyond the limits of this review. It seems clear, however, that whether the authors realize it or not, the fundamental question is whether the quantitative approach to social behavior by way of the stimulus-response formula has proved to be effective. They are shifting the emphasis to the organism where it properly belongs, and they are coming to realize more fully that the stimulus is only one of many factors which condition a response.

Dashiell's review is restricted in the main to the experiments "upon the effects of *groups* of people upon the individual person." He thinks it "more worth while to present a running critical description of the more outstanding studies in their variety than to force them into refined classifications of problems or procedures, or strain for generalized deductions and conclusions where such would be utterly unsafe." In this way he takes up in order the various experimental studies dealing with the effect of spectators or auditors, of co-workers, of competition, and of group discussion upon the individual's work, and of majority and expert opinion upon the individual judgment. Concerning some of these problems the author presents details of experiments of his own which heretofore have been published only in abstract form. In general his "groups" are merely aggregates which may differ in size and constitution, and the "effects" are always determined by the performance of the individual. He realizes the limitations of the method as regards social psychology at large, and he regards the experimental attack of these problems as preliminary, but he thinks that the results thus far obtained indicate the future usefulness of the method in this field.

I suppose that my notion of what constitutes a good handbook article derives from my acquaintance with Wagner's, Hermann's, Nagel's and Graefe's handbooks of physiology, or from the *gesammte Referate* that in former years occasionally appeared in the *Archiv*, or from the general review that Binet and Victor Henri published in

*L'Année*. Helmholtz called his "Optics" a handbook, and most of Wundt's *Grundzüge* is of the handbook type. Despite differences, every article or chapter is a survey of the field designated by its title. Problems, methods, results, and theories are all viewed dispassionately, tolerantly, with understanding, with comprehension of their inter-relations and their significance for the topic as a whole; they are sifted, weighed, evaluated, generalized and set forth with some artistry as regards composition, tone and perspective. By 'tone,' in this context, I mean dignity, sincerity, tolerance, caution, accuracy or, in a word, scholarship; it implies also an exposition at the level of a reader who has some competence in the field in which the article or chapter belongs; in general the article is thoroughly documented.

With *credo* stated, I am free to say that the articles by Esper on *Language*, Willoughby on *Magic and Cognate Phenomena*, Sheldford on *The Physical Environment*, Wallis on *Social History of the White Man*, and Schjelderup-Ebbe on *Social Behavior of Birds* do not belong in a handbook; no one of them is a comprehensive survey of the field indicated by its title. All of the other articles approximate in greater or less degree to the criterion. Much of Plath on *Insect Societies* is, it is true, an old story to any social psychologist, and it does not faithfully present the present status of the subject. Wissler's two articles, while comprehensive and authoritative, are written at the level of an encyclopedia instead of that of a handbook. Alverdes on *The Behavior of Mammalian Herds and Packs* is naïve as regards the origin of the statements he sets forth as facts. Nowhere is scientific observation more difficult; every statement should have been weighed with the greatest care and then documented. These are the lesser approximations; I have commended the others in their places.

If a single article should be comprehensive as regards the field indicated by its title, the handbook taken as a whole should be inclusive of the field covered by its title. A handbook of social psychology should therefore contain nothing that does not relate to social psychology, and nothing should be omitted that would add to a comprehensive view of the field as a whole. Regarding the former, I have been able to justify all of the *titles* save one—*The Physical Environment*. But in some instances the substance beneath the title has given me real difficulty. The contribution to social psychology of the two 'population' articles is not great. My mistrust of social analogies among plants, and my inability to see

what regression has to do with social psychology makes me question the inclusion of the Clements, Willoughby, and Wells articles in this *Handbook*. I can accept the cultural histories because they offer an excellent background, and indirectly they call the attention of social psychology to a neglected field. But I think the editor would have done better by us if, instead of the cultural histories of the white and yellow man, he had given us two papers on typical savage cultures prepared by writers of unquestioned authority who could have told us where cultural anthropology merges into social psychology. The *Handbook* by title at least does full justice to the social psychology of animals; it does not do so well with that of man.

What, then, of the omissions? There are, for a comprehensive survey, three serious ones. There should have been an article on the present conception of what social psychology is. If there are many different conceptions, that is all the more reason why they should be collected, examined, and compared. If it is impossible at the present time to agree on a definition, the field can at least be limited. This *Handbook* is itself witness to an extraordinary confusion between the *social* and social psychology; between cultural and psychological, even between individual and social psychology. In the second place there should have been an article on human socialization, upon the conditions, the factors that guide or govern the individual organism in its social life. The writers on the social psychology of animals are not unmindful of these things, but those on human social psychology deal almost solely with the *products* of socialization and deal with these as the results of social situations or stimuli with occasional reference to age or sex. Does it mean nothing that the infant is born in a social environment and for a dozen years or so is dependent upon his elders; is his racial stock of no significance; does age mean no more than a time when one begins to smile or laugh or learn a language; does sex do no more than create a need for adjustment? At least partial answers to such questions as these are in the literature, and they should have been swept together. The *Handbook* should also have included an essay on the human *group*. With the single exception of the Yerkes article, every one of the animal articles treats social behavior solely in terms of the social organization of a number of individuals, *i.e.* of group behavior. Consequently, interaction between individuals is not considered as social at all. The human psychologists, on the other hand, deal solely with individual reactions, adjustments, responses to other individuals or aggregates of individuals. Why

is the group as a configuration ignored? There is a not inconsiderable experimental literature on the group, and it is the group that furnishes the bridge to cultural anthropology. The omission of the topic is unfortunate.

Among books the handbook is the tool par excellence. But its usefulness depends upon what one finds when one turns to it. If one finds many things that are unexpected, that perhaps is his gain. It is what one expects and does not find that disappoints. In this review I have endeavored to indicate what may and what may not be expected from this *Handbook of Social Psychology*. It has not proved to be the comprehensive survey of the field that we should like. But if we disregard the title and consider the book as a collection of essays which in one way or another have a bearing upon what may be called the social, then, with possibly one or two exceptions, every article in it is in its own way useful, and many of them are unquestioned contributions to social psychology.



## BOOK REVIEWS

HARTMANN, GEORGE, *Gestalt Psychology*. New York: The Ronald Press Company, 1935. Pp. xiii+325.

In *Gestalt Psychology*, Hartmann has written a book that is sure to cause the reviewer trouble, for he has performed his task with such competence and skill that he is repeatedly on the point of rising above competence and skill, and is therefore continually tempting the reviewer to judge his performance by the standards of a category to which it does not belong and does not pretend to belong. To succumb to the temptation would be manifestly unjust. Yet the temptation exists in a dangerously insidious form: it is impossible to indicate with any definiteness the level to which the book rises without at the same time calling attention to the level it does not reach.

Perhaps the best way of dealing with this difficulty is to let the author speak for himself concerning the purpose of his book. In the preface he says:

"This volume is an attempt to bring together in convenient form all the material necessary for a more than superficial understanding of the subject with which it deals. It is intended to be read with profit by anyone who has had a first course in elementary psychology. Essentially, I have aimed to give a sympathetic picture of the Gestalt system from the standpoint of a non-configurationist, although I must confess that an examination of the evidence has left me more favorably disposed toward the theory than I had originally anticipated. My major task has been that of an expositor and interpreter and only secondarily that of a critic. Had my equipment and resources been adequate, I should have preferred to make this work a complete reference handbook, but external limitations compel me to rest satisfied with a less pretentious, but I hope equally serviceable, summary and orientation."

Without any hesitation it can be said that the author has accomplished his purpose. As a serviceable summary, as a useful means of orientation, as an exposition and interpretation leading to "a more than superficial understanding," the book is an admirable achievement. Its five main divisions—historical, theoretical, empirical, practical, and critical—present the material in such a way that the reader has no one but himself to blame if he does not soon find himself acquiring a feeling of familiarity for the world of the Gestaltists. A great merit of the book is the definiteness

with which the subject matter is presented; its pages are full of specific instances and concrete details. There is an adequate supply of the drawings without which no exposition of Gestalt psychology would be complete—the arrangements of dots and lines and figures which picture the principles of the school in black and white—and the typical experiments are reported in sufficient detail to give the work body and substance. Furthermore, as each concept, each experiment, each point of view is presented, the author does his utmost, either in the main text or in the plentiful footnotes, to indicate clearly the lines of relationship to other concepts, experiments, and points of view. In doing so he draws on a large fund of information, and if he omits some of the studies one would naturally expect—it is particularly difficult to justify, in a book for American students, the omission of Duncker's work on thinking and Klüver's experiments on brain mechanisms—the remarkable fact, considering the scope of the task, is not that something has been omitted, but that so much has been included. The book above all has been built for service. It is fully equipped with such convenient devices as a systematic table of contents, lists of illustrations and tables, a note on bibliographies, brief biographies of some of the leaders of the movement, a chronology of significant dates and events, a glossary, and a careful index. Everything has been done to make the book a useful handbook. And it is not merely useful and not merely a handbook. It has vigor as well as clarity; it is shrewd as well as painstaking; it is stimulating as well as informative.

But it would be misleading to stop at this point. For while one is admiring the competence and downrightness with which the author treats the material in the foreground of the system, one becomes aware of the fact that he does not work with his usual perspicacity in dealing with the more remote implications of the facts and principles that first claim attention. In his eagerness to get the outstanding concepts down to clear-cut terms and to make the main lines of relationship stand out with unambiguous distinctness, he sometimes permits the more subtle, but not less important relationships to escape him. It is difficult to illustrate adequately so general a characteristic, but perhaps as good an instance as any is his treatment of some of the anticipations of Gestalt psychology. In his comments on James and Dewey, for example, he almost gives the impression that they are Gestaltists; yet such an impression is patently false and is unjust to all parties concerned. Of course Hartmann knows as well as anyone else that James and Dewey are

not Gestaltists, and perhaps he considers the point so obvious as to require no comment. Yet it is significant that the shades of difference that lead to such divergent positions do not greatly interest or concern him. Evidently neither his usual ardor nor his usual skill is aroused by remote implications. Sometimes he even misses the not too distant implications of his own remarks. He says, for example, in writing of Lewin, "A victim of the Aryan policy, he now holds a lectureship at Cornell," and there is nothing in the tone or context to indicate that the possible implication about Cornell is intended. Not that Hartmann is above having his joke, but his jokes do not depend on hidden implications. His humor is hearty and aboveboard and forthright—as direct and explicit as his exposition. When he intends an implication he calls attention to it openly, as in the statement, "Gestalt theorists appear to be guilty of a lack of insight (!) into the defects of their doctrine."

This point is not so trivial as some of the illustrations might suggest, for it is this lack of regard for the less immediate relationships that keeps the author from achieving a conception of his material that would enable him to see it as a whole and at a distance. He has thrown himself into his task with a zest; he has not stood off from his material and surveyed it contemplatively. It is significant that the critical section of the book is the least satisfactory, and that after stating the positions of the best known critics of the Gestalt movement, he can present his own views in a chapter of less than two pages and can sum up his attitude in the words of another man. To be sure, the author has intentionally set himself a task which, in his own words, is only secondarily that of a critic, and he can hardly be blamed for consistently following his own plan. But the fact remains that the lack which makes itself felt most keenly in the critical portion of the book is not without its effects in the expository chapters. Occasionally something is said or left unsaid that puts a statement slightly off key or even makes it definitely jarring; there are evidences now and then of obscure torsions of meaning below the surface which, if straightened out, would alter the course of the discussion. For exposition no less than criticism requires an envisagement of the subject-matter that takes into account its ramifying implications and its essential wholeness as well as its salient features and the definite pattern of their arrangement.

But again it would be misleading to stop at this point. It would give an unjustifiably false impression of the book to close the dis-

cussion without calling attention once more to the fact that it is the high level of performance that the book maintains that makes one so conscious of the level just above it. The skill, the thoroughness, and the honesty of the book make one expect it to reach the mark one is disappointed to find that it misses. An ordinary book would arouse no such expectation and consequently would provoke no such disappointment.

EDNA HEIDBREDER.

Wellesley College.

BENTLEY, ARTHUR F., *Behavior, Knowledge, Fact*. Bloomington, Indiana: The Principia Press, 1935. Pp. xii+391.

Although many books on the methodology of the social sciences have appeared recently, those based on a sound philosophical background are rare indeed. Even among the philosophically sound, Bentley's book stands out for its stringent logic, its coherence, and its comprehension of the scientific method. Although Bentley may not appreciate my calling his book philosophical (he disdains the word), I do so advisedly. By philosophy I mean philosophy of science as practiced by the Vienna neo-positivists and the better equipped adherents of the American Philosophy of Science Society. Philosophy to these individuals means critical philosophy, logical investigation of science as language, and comparative theory of science (*vergleichende Wissenschaftslehre*). Among studies in the *vergleichende Wissenschaftslehre* of the social sciences Bentley's book may well be a landmark.

The subject-matter is very difficult: This is not a book for social scientists to page through on rainy Sunday afternoons. It must be read and reread. The footnotes may not be passed over. One should even check back on the cross references. It would be an admirable text with which to weed out undesirable graduate students. But read carefully by many students of social science it may well have a decided salutary effect. Methodological insights will be gained; pseudo-problems will disappear; social postulation, social observation, and social fact will become better integrated. The difficulty of the book comes from both its subject-matter and its method. The subject-matter is the methodology of social science. The method is an examination of the coherence of social science postulates. From the 'fact' that 'words' designating 'facts' have no one-to-one correlation with the 'facts' designated, Bentley is forced to invent a rather complicated (for psychologists

one might add Tolmanesque) nomenclature. This precludes giving an accurate abstract of the argument within the compass of a brief review. The following lines attempt only to indicate the approximate content of Bentley's argument. It is hoped they will lead some psychologists to the careful study of the book.

The book is divided into three parts. Part I, "Psychology as Knowledge," starts by pointing out, and on this point most methodologists will agree, that all science is description, but description in specialized languages. He quotes from Titchener, "All science is talk, but not all talk is science." His primary concern then is to examine contemporary psychologies as languages, with particular regard to their linguistic coherence. He distinguishes two "dominant language techniques," the Aristotelian and the scientific. This distinction is very close to what Lewin has called the Aristotelian and Galileian modes of thought and what the present reviewer has called class theory and field theory. The concepts are only introduced here and we shall return to them in discussing Part II. He further distinguishes two "linguistic differentiations in psychology," the mind-language and the physical language. The distinction here is close to Carnap's "language of data" and "language of constructs" and to Lewin's "phenotypical description" and "genotypical description." Since the physical language in modern science approaches the scientific language technique the "issue for psychology then becomes one as to whether the new physical language will develop so as to cover the phenomena of psychology or whether the old mind-language may transform itself into an adequate medium for this purpose, with the further possibility that in the outcome the two languages may fuse into a higher consistency than either of them can attain separately." Finally psychologies must be examined with regard to their treatment of spaces and times. From these three aspects of linguistic technique 11 systems of psychology are examined. The outcome: No one graduates "summa cum laude" and about 60% fail. From the standpoint of approaching adequacy of scientific language M. Bentley and John Dewey come out on top. From the standpoint of overcoming the deficiencies of the mind-language the Kantor of 1924 comes out on top. The Kantor of 1933 fares even better because his treatment of psychological space-time also approaches adequacy. No one, however, has presented a system of psychology which is "scientific" in all its aspects. Lack of space forces the reviewer



to advise other psychologists and their adherents to look to Bentley's book for their ratings.

This first section is full of the keenest methodological insights and devastating criticisms. I know of no other survey of modern psychologies which so thoroughly examines the linguistic adequacy of psychological theories. There are points to be made in criticism however. The choice of psychologies (chiefly from Murchison's of 1925 and 1930) is highly arbitrary. These symposia are rather a hodge-podge at best with the essays very uneven in aim, scope, and competence. Many of the contributors undoubtedly simply used the occasion to publish one more theoretical essay. This has led to the arbitrary rejection from consideration of several individuals who might well have been more seriously considered. Further, Bentley's acquaintance with Gestalt theory is unfortunately slight. He devotes a note to Lewin in which he calls Lewin's psychology "pure Kantor." This is highly cavalier. The reviewer believes Lewin should also be allowed a strain of Dewey—M. Bentley (in A. F. Bentley's sense) and hence be moved to the head of the class, as scoring highest in both parts of the examination. Lewin also as an individual has given the most serious consideration to problems of psychological space in his development of topological psychology. Bentley scarcely touches on this aspect.

Part II is chiefly concerned with the "inquiry into a form of postulation which will permit the study of men and things in systems, in a manner in harmony with those other forms of research and knowledge that are before us in the old established sciences." In other words the chief subject-matter is the scientific linguistic technique and its differentiation from the Aristotelian. The relationships between language, knowledge, fact, and experience are investigated linguistically and methodologically. Language and knowledge, like experience and fact, are both behavioral and neither exists without the other. There are two forms of fact, brute fact (*i.e.* raw fact, immediate fact, empirical fact) and the fact of scientific language development (*i.e.* the fact of advanced scientific construction and organization). Roughly speaking the scientific process strives to translate or order the brute facts to the scientific facts. Readers familiar with modern writing on method will recognize the similarity here to Lewin's "phenotype" *vs.* "genotype" and Carnap's "language of data" *vs.* "language of construct." Bentley is cognizant of Lewin's distinction but claims his own to be much more basic,—deeper rooted, so to speak. In this the reviewer is unable

to follow him. Be this as it may, he establishes valuable criteria for social fact. Social fact if it exists must be visible, factual, and behavioral, *i.e.* distinct from either the 'physical' or the 'vital' (biological).

Part III uses the methodological insights of Parts I and II in making a search for certain basic scientific social facts. One such fact is the social event of the conversational remark. When one individual addresses another and the other responds, we have an event which is a social fact. It is observable, factual, and cannot be accounted for with the existing physical or biological space-time constructions. From this fact Bentley develops a set of postulates and definitions to deal with the social problem of communication and the psychological problem of perception. A whole new and very precise nomenclature is developed, which defies abstraction. The reviewer will hence not attempt any account of these chapters (XXIII-XXVI). From this we are led to an illuminating discussion of the psychological and sociological as categories. This discussion is one of the high points of the book. Volume after volume on this subject-matter now becomes obsolete. There is no behavioral event which is not both psychological and sociological. The great potter about whether psychology or sociology is a "basic" science turns out much ado about nothing, a meaningless debate. Neither is basic to the other. But both are respectable realms of discourse. To put it crudely, when the interest is in the *perceptions* of the individual in *psychological space-time* the problem is being investigated psychologically. When the interest is in the *communications* between two or more individuals in *sociological space-time* the problem is being investigated sociologically. (The italicized words refer to the approximate popular terms nearest to the precise definitions of Bentley.) Hence the psychological and the sociological are simply aspects of human behavior, both always present, but presenting two possibilities of attack. In this discussion Bentley shows a high disdain of social psychology, which he considers a bastard social science. The reviewer wonders if perhaps social psychology cannot be written as the science of what Bentley calls behavioral space-time. The book ends with a chapter showing that the dichotomy of pure and applied sociology is meaningless. With this view-point the reviewer heartily agrees.

To sum up, Bentley has given us a linguistic analysis of modern psychologies, established a scientific category of the behavioral, and shown us its applicability. The book is a methodological treatise

of a 'philosophical' nature. In this point lies both its strength and the source of a certain disappointment that the reviewer felt on its completion. It would be, of course, unfair to expect a presentation of a 'scientific' sociology from a book which only pretends to be a prolegomenon for all future ones. But one still is unable to suppress the feeling that we need the science today rather than the methodologies. Bentley, to be sure, touches on the problems of the state, money, and the economic order, and looks forward to their possible solution. But the world is crying out for the solution. Let Bentley's next book be on sociology rather than method.

Against this Bentley may reply that we cannot sow the seed until the field be plowed. This is granted; the question is what type of work best plows the field. What I mean may perhaps best be exhibited in the form of personal anecdote. The reviewer has had many and long arguments with both philosophers and scientists concerning the fruitfulness of philosophy of science and particularly the logical analysis of scientific language. In these arguments the position of most scientists is about as follows:

"These studies and treatises on linguistic analysis and methodology simply clarify method after it has been developed in the sciences proper. It is perfectly true that the vast majority of scientists are illiterate as far as the language of science is concerned and that these philosophers whom you admire can see this illiteracy and analyze it. But the illiterate scientist through his illiteracy is unable even to read these treatises when they appear. The literate scientist reads them but he does not need them. Consequently they are without real import." In these arguments I have (usually) taken the philosopher's side, namely that such works as Bentley's have a real value in advancing scientific knowledge. I may close my review with the honest wish that Bentley's book be widely read and lead to results which show me to have been right in my side in the debates.

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LEWIN, KURT, *A Dynamic Theory of Personality*. (Trans. by D. K. Adams and K. E. Zener.) New York: McGraw-Hill Book Company, 1935. Pp. ix+286.

This volume, intended as a summary presentation of Lewin's systematic position, is composed of eight papers previously published

elsewhere, two in English<sup>1</sup> and the remaining six in German. It is difficult to judge the adequacy of a translation without having read the original but internal evidence indicates a high degree of care and thoughtfulness on the part of Drs. Adams and Zener; certainly few systematic writings have been as attractively presented in translation as have these. Obvious attention has been given to the difficult tasks of making the English readable and of linking the sometimes difficult concepts with terminological usage of American psychology.

A book at least as long as Professor Lewin's would be required for an adequate critical examination of his position. The whole approach is tremendously vital and has already proved fertile as a point of departure for the experimentalist. In his preface, Lewin says, "After the initial successes of experimental psychology in its early stages, it seemed to become clearer and clearer that it would remain impossible for experimental method to press on beyond the psychology of perception and memory to such vital problems as those with which psychoanalysis was concerned. Weighty 'philosophical' and 'methodological' considerations seemed to make such an undertaking *a priori* impossible." The present book serves as a brilliant and successful rebuttal to those "weighty considerations." As a whole the emphasis is laid on social and emotional behavior with the problem of motivation as a keystone, the child being considered most extensively because of his greater availability as an experimental subject.

Several aspects of the book, however, will undoubtedly trouble psychologists of other systematic persuasions. There is first of all unfortunate evidence of the "straw-man compulsion" so characteristic of theoretical writings in psychology. Few systematists have been able to present their own positions without setting up a hypothetical group of "other psychologists" whose beliefs are contradictory (and curiously stupid in contrast) to their own. The "Aristotelian mode of thought" serves as a foil for Lewin in much the same way the "introspectionist" and "machine theorist" served Köhler. The inaccuracies of Lewin's conception of current modes

<sup>1</sup> The Conflict Between Aristotelian and Galileian Modes of Thought in Contemporary Psychology. *J. Gener. Psychol.*, 1931, 5, 141-177; Environmental Forces in Child Behavior and Development. Chap. 14 in Murchison, C. (Ed.), *Handbook of Child Psychology*. (Second Ed. Rev.) Worcester: Clark Univ. Press, 1933.

of thinking have been pointed out elsewhere.<sup>2</sup> Needless to say, these inaccuracies do not in themselves invalidate Lewin's own position.

The use of the term "dynamic" is open to a similar criticism. It has been the usual fate of psychological systems to begin (historically) with commendable interest in the larger and somehow more "human" problems and to dwindle eventually, perhaps for laboratory reasons, to minute problems having little bearing on the original ones. New systems arise in opposition to this dwindling and, to get back to "human nature," proclaim themselves *dynamic*. Freud and Morton Prince revolted against the sterility of conscious content schools; the Gestalters have revolted principally against mechanistic behaviorism. It is clear from the writings of Watson, Weiss, and others, however, that behaviorism was originally just as much concerned as is Lewin with the larger and more complex psychological problems of man in his social environment.

It appears from Lewin's writings that to him *dynamic* means an emphasis on the changing relationships between the organism and its environment, fluid changes dependent on the ever-shifting condition of both the environment and organism. Just why this emphasis makes his system dynamic and behaviorism, by implication, static is hard to see. Recent writings on the same subject-matter by American reaction psychologists have not, it is true, dealt in such detail with problems of conflict, detour behavior, and social polarizations. This would seem to be a function of the personal interests of the psychologists, however, rather than a limitation of their systems. It is perfectly possible, as French,<sup>3</sup> Stogdill,<sup>4</sup> and Sears<sup>5</sup> have demonstrated, to attack problems of psychopathological significance from a strictly mechanistic view-point, and to construct hypotheses based on habit or conditioned response principles which account for the fluid or dynamic characteristics of behavior as adequately as do hypotheses based on "dynamic theory."

Finally, and of greatest importance, some psychologists may question the advantage of Lewin's concepts of *field forces* and *valence*. If a hungry child sees a chocolate across the room he approaches it;

<sup>2</sup> Margineanu, N., Professor Lewin's Conception of Laws. *J. Gener. Psychol.*, 1935, 12, 397-415.

<sup>3</sup> French, T. M., Interrelations Between Psychoanalysis and the Experimental Work of Pavlov. *Amer. J. Psychiat.*, 1933, 12, 1165-1203.

<sup>4</sup> Stogdill, R. M., Neurosis as Learned Behavior. *Psychol. Rev.*, 1934, 41, 497-507.

<sup>5</sup> Sears, R. R., Functional Abnormalities of Memory with Special Reference to Amnesia. *Psychol. Bull.*, 1936, 33, 229-274.



if there are obstacles which force him out of a direct pathway he goes around them. He is said to be passing through a field of positive and negative valences. The objects, in other words, have forces which attract or repel him. Lewin's description of such a situation would be given in terms of these valences and field forces; the chocolate would have a positive valence for a hungry child and a negative or zero one for a chocolate-satiated child. The changes are described in terms of the chocolate rather than the child and psychological qualities are attributed to it in the same way the chemist attributes a physical quality (*e.g.* atomic weight) to a metal.<sup>6</sup>

It is true that such a physical quality as atomic weight may be determined only by proper measuring instruments and if two of these instruments differ, different atomic weights will be obtained for a single metal. The chemist making such a measurement, however, is interested in the metal, not in the instrument. By rights the psychologist should be interested in the child (instrument) not the chocolate (metal). Thus, to some psychologists it may seem more appropriate to limit description and explanation to the reaction systems of the organism rather than to involve themselves in an ever-changing relationship between two variables of (at any given moment) unknown characteristics. There is, further, the danger of developing a wholly artificial dichotomy between perceptual systems and reaction systems. The psychological valences of objects are dependent on the "perceptual field" and the organism *reacts* to this field. Lewin says, in fact: "Thus there occurs a *steering* of the process by a perceptual field" (p. 48). To some it would seem preferable to say that *perception is part of the total reaction system*. For example, the excitatory tendencies stimulated by chocolate are, in a hungry child, reinforced by excitatory tendencies stimulated by the condition of hunger; in a chocolate-satiated child they are inhibited by contradictory excitatory tendencies stimulated by the condition of chocolate-satiation. The perception of the chocolate as a "to be approached" or a "to be avoided" object is dependent on the initial reaction systems set in motion by the dominant excitatory tendencies. By such a description as this the emphasis is laid on the aspect of the total situation (organism) in which differences may produce different behavior. The chocolate is always a chocolate

<sup>6</sup> For a clear and unambiguous statement of this position, see: Higginson, G. D., *A Systematic Approach to Psychology in Terms of Function and Product*. *J. Psychol.*, 1936, 2, 1-23.

physically and it gains its psychological properties not from a disembodied "perception" but from a total organismic response to it.

These points of negative criticism will seem of small importance to the experimental psychologist struggling for methods in this field. The points of positive value are far too numerous to mention in a brief review. As a contribution to the study of personality, social and abnormal psychology, and the psychology of motivation this is a work of the first importance and deserves to rank with such great historical contributions as those of Freud and Prince. It serves, too, to make clearer the distinctive differences between the Gestalt and behavioristic approaches to these problems. If a book of equal strength can be presented by a behaviorist the battle-line will be fairly drawn and an ultimate decision as to the preferable systematic approach will be in sight. Professor Lewin's book, to a greater extent than any other appearing in the last decade, is required reading.

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DOOB, L. W., *Propaganda, Its Psychology and Technique*. New York: Henry Holt and Company, 1935. Pp. x+424.

At least since the World War, treatises on propaganda have been considered timely. The problems of this field have been attacked by the historian, the political scientist, the sociologist, and, more recently, by the educator and the psychologist. At first the methods employed were of the anecdotal sort; and even yet they are but rarely experimental. Practically all modern social scientists agree that propaganda must not be defined in ethical terms. On other items there is far less agreement. In fact the very definition has little scientific definiteness.

Doob, in a book dedicated to his teacher, Gordon Allport, has defined his subject rather broadly. For him "Intentional propaganda is a systematic attempt by an interested individual (or individuals) to control the attitudes of groups of individuals through the use of suggestion and, consequently, to control their actions; unintentional propaganda is the control of attitudes and, consequently, the actions of groups of individuals through the use of suggestion" (p. 89). Education is "the imparting of knowledge or skill which has reached the scientific stage or of scientific procedures" (p. 80).

To the reviewer these definitions, while reasonably serviceable, do not possess sufficient objectivity. The term *intentional* seems the least satisfactory element. At best one can only say that in inten-

tional propaganda the propagandist is behaving as *I* would were *I* intending to do thus and so. And when Doob, in his illustration of some point, mentions the *survivors* of a California earthquake, how should a sensitive Californian interpret him? Was Doob, a resident of New England, *intending* to convey the idea that the loss of life in California earthquakes is generally heavy, or was he issuing mere *unintentional* propaganda? Doob himself admits that his classifications overlap to some extent; and, in fairness to him it must be admitted that no absolutely clean-cut types of propaganda have ever been offered by anyone.

Doob's decision to confine the term "education" to a small portion of what the layman ordinarily considers it to be appears to the reviewer a little extreme. Rather than accept this view, the latter suggests the treatment offered by R. T. LaPiere.<sup>1</sup> This sociologist, while indicating his desire that the terms "education" and "propaganda" be dropped, if possible, from scientific usage and although declaring that adequate psychological distinctions cannot be made, states that a sociological distinction is possible. He suggests that the term "education" be employed whenever there is social pressure ("intentional" or "unintentional") to conform to the behavior patterns of the majority of the group. Propaganda (as a sociological matter and not as a psychological process) then could refer to situations in which there is pressure to conform to the behavior norms of some minority. Even this distinction does violence to the lay notions and does not always offer a clean-cut dichotomy; yet it furnishes a fairly workable schema.

In Part 1 of the Doob treatise the chapter headings include "The Search for Weapons" and "But is Psychology Useful?" In Part 2, "The Psychology of Living People," Doob discusses motivation, attitudes, stereotypes, personality and society, and suggestion, prestige, and social change. Borrowing from the attitude classifications of H. Cantril, Gordon Allport, and others, Doob builds a most interesting logical structure to prepare the reader for his eight major principles of propaganda. These are discussed in Part 3 under the titles: intention, perception, type (revealed, delayed revealed, and concealed), related attitudes, desired integration, sphere of unpredictability, counter-propaganda, and persuasion. In Part 4 Doob considers commercial propaganda, propaganda societies, the Communist Party of America, Nazi propaganda, and questions regarding

<sup>1</sup> LaPiere, R. T., *Propaganda and Education. The Need for a Quantitative Distinction. Sociol. & Soc. Res.*, 1935, 20, 18-26.

war and peace. These topics appear to be well handled, although it probably can be fairly said that for Doob propaganda has not been successful unless there are tangible results. He seems to forget that many of our peace efforts, for example, may bear fruit only at a much later date. Part 5 is concerned with vehicles of propaganda—the newspaper, the radio, the motion picture, the stage, the other arts, and the several minor vehicles. He concludes his book with a very provocative chapter in which he clearly indicates the dilemma of the liberal, the true social scientist. The eight principles (references to which are generously given in the latter half of the book) are listed in expanded form in the appendix.

Doob has given us a most interesting and readable book. To the majority of readers the second half will naturally prove of greater interest than will the first. In fact a large group of serious readers will wish to consider the second half of the book first, then the first half, and later the second half again. This procedure will make a bit clearer the logical need for the extensive motivational analysis found in Parts 2 and 3.

"But," the reader may ask, "is Doob's *particular* type of motivational analysis necessary for an understanding of propaganda?" The reviewer answers in the negative. It is quite conceivable that some student of W. I. Thomas might give us a splendid framework in terms of wishes for our studies of propaganda. Similarly, Lewin or one of his disciples might employ topological analysis to advantage in this connection. In other words, any particular motivational analysis furnishes but a scaffolding which will aid in the strictly behavioral analysis to come later when the phenomena are better understood. Several somewhat rival systems may be almost equally adequate for this preliminary descriptive work.

It would appear that Doob understands at least to some extent the limited status of motivational analysis. This can be deduced from his handling of Biddle's research. For, after heavily criticizing Biddle's theory of emotional conditioning (the reviewer could not follow the criticism in its entirety), Doob says, "Most of the examples that have been discussed can be treated in accordance with either theory; the theory proposed by the writer, however, seems more reasonable in many instances. In the last analysis a series of crucial experiments must determine which theory possesses the greater validity" (pp. 123-124).

One would be fairly safe in predicting that Doob's book will have a wide circulation. While the experimental literature is not covered

as extensively as the psychologist might desire, the problems have been given careful thought. The illustrative material is both interesting and pertinent. The book can be read with profit both by the layman and the social scientist.

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LATOUR, MARIUS, *Premiers principes d'une théorie générale des émotions. Nouvelle édition revue et augmentée.* Paris: Félix Alcan, 1935. Pp. 650.

One may judge the generality of this theory by two questions the author asks in the beginning of the book with the prediction of an affirmative answer. Is there a single formula which will encompass beauty, goodness and truth, aesthetics and ethics, and all the different philosophical conceptions thereof, and which will be satisfactory as psychological analysis? Is it possible to construct a similar formula for laughter and the comical? The immediate problem the author takes to be that of the fundamental cause of emotions, the spontaneous products of sensibility that coexist with perception and develop prior to reflection. (He rejects the peripheral theory of emotion as having no relevance to cause.) His purpose is to determine by means of introspection the element which represents the origin of emotion for nonreflective awareness (*imagination*).

Latour's theory is a systematic voluntarism, the origin of which he traces to Maine de Biran. "Introspection" apparently means for the author a form of deductive reasoning following the pattern of Hegelian dialectic, for the presentation consistently moves in triads of thesis, antithesis, synthesis. The most ubiquitous psychological object is will, the manifestations of which appear in contrasting pairs which analysis soon submerges in a fundamental unity; these units also contradict one another in a hierarchic fashion, so that after all everything is swallowed up in the pervasive will. This view of the theory, however, only occurs to the reader upon finishing and is unlikely to diminish his enjoyment or final evaluation of the book. The author makes admirable use of voluntarism to bring intelligibility into the manifold of human sentiments. He analyzes emotions at a rate per page that confounds the reader's ability to recall particulars, but which leaves him with a firm grasp of basic principles.

According to Latour, all emotions can be reduced to this simple formula: given a volition, its success arouses pleasure; its failure arouses pain. This volition may be either the individual's own or



that of another (external will, which is really the former projected). With respect to external will one may feel either of two sentiments, solidarity or opposition. Thus by combination four simple emotions emerge: in the case of solidarity, affirmation and negation arouse respectively *admiration* and *compassion*; in the case of opposition, affirmation and negation of a volition arouse *vexation* and *laughter*. In actuality combinations of these are experienced as one emotion, since it is possible to feel in two different senses both solidarity and opposition. If we symbolize these four simple emotions as A, B, C, D, in the order above named, the following complex ones may be formulated. *Respect* equals A plus C; *pity*, B plus D; *benevolence*, A plus D; *disgust*, B plus C.

Implicit, as well as explicit, affirmation or negation is capable of causing emotion. Will, the abstract side of emotion, is implied in four forms, which the author calls "attributes of will." They express, in the following order, increasing degrees and levels of abstraction: (1) desire (dependence upon physical pleasure and pain), (2) effort (economy of will or dependence upon utility), (3) pursuit of ends (negation of utility and dependence upon moral pleasure and pain), (4) liberty (attainment of complete independence of circumstances). After carefully identifying these levels, for they are the paradigm upon which the remainder of the book is patterned, the author formulates a complete statement of his theory. "*According to our theory, the emotions are aroused either by the success, the affirmation of a volition, the revelation of the attributes of will or a higher degree of will; or by the failure, the denial of a volition, the negation of the attributes of will or again the revelation of a lower degree of will*" (p. 52).

Anticipated failure or success (fear and desire for assurance) is fundamental in the sense of being related to all other emotions (p. 608). Fear, in some form or other, is at the origin of every emotion except laughter; it is evidence of an object's capacity to favor or hinder and thus gauges its reality. Laughter itself is the negation of fear (*peur manquée*). Latour disagrees with Bergson who opposes laughter to emotion as intelligence is opposed to passionate, nonreflective awareness, implying that a man of pure intelligence would still experience laughter. According to Latour, laughter is the negation of external will and is thus a substitute for any other emotion. It is always accompanied by assurance. To deny all emotion would be to deny the very basis of reality (will), including the individual himself, and this prospect would not be mirthful.

'Will, in its various forms, is not explicitly present in the consciousness of every-day man. Just as his correct use of words in speech implies their grammatical forms, man's reactions *intuit* a world of volitions (p. 585). Precise statement of the machinery of will in the origin of emotions is the rôle of the theory; it proposes to be a grammar of the emotions.

The main body of the book, pages 55 to 583 (Chapters II, III, and IV), is a systematic development of the implications this theory has for all the categories of human activity, making it of direct interest to students of economics, politics, ethics, religion, sociology. The sequence ascends the hierarchy of the degrees of volition. Reality and beauty, the culmination, are presented as the same thing and based upon emotional experience. Aesthetics, according to Latour, is the one universal science. The rôles of art and the comic are the creation of the illusions respectively of reality and unreality.

In verifying his deductions the author introduces numerous illuminating analyses of factual materials which have explicative value apart from the system as a whole, and which constitute the real merit of the book. These sources of verification are threefold: authoritative opinion, emotional life of primitive man, and mental disorders. The pages of the book are replete with quotations from poets, literary men, and philosophers, primarily of the nineteenth century, and show a breadth of scholarship which is distinguishing in any branch of writing. While evidence of this kind need not be considered final, it should rightfully be taken into account in a complete theory. Latour's sources on primitive and abnormal mind are chiefly Lévy-Brühl and Pierre Janet. In the mind of the primitive, dominated by the animistic conception of the universe (*mana* and *miasma*), he sees the working of pure *imagination* prior to the development of abstract reflection. Mental disorders are explained as various aberrations of will. The author does not systematically review this valuable material, but scatters the countless pieces along as he fits them into his main outline of the degrees of reality. And the reader is left to compile his own index.

It is difficult to do justice in summary to a book of this kind, and easy to pick flaws with minor details. There is only one reference to relevant experimental work in this country (p. 51), and this is not considered a major contribution to the study of emotions. Questionable psychological laws are introduced as explanatory principles, such as, for example, the law that in the psychic realm there is a repetition of cause in its effect (pp. 58, 93, 328). Latour's use of

"instinct" in more than one sense (fictitious and physiological) and interchangeably with "sentiment" is confusing to the reader. A more serious criticism is that the author makes no consistent attempt to clarify the genetic priority of the many instincts, sentiments, forms of will, which are integral parts of his system. Another source of confusion is the religious tone which at times makes the reader wonder whether the author is telling what ought to be or what is. Latour's postulation in man of "a permanent desire to see accounts balanced" (moral reparation) (p. 331) is likely to grate on the nerves of an American psychologist.

While the average psychological reader may justifiably feel that this theory is not completely satisfactory to "psychological analysis," he should henceforth recognize the woeful insufficiency of any theory of emotion that does not *in some fashion* include the dominant position of will.

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DUNBAR, H. FLANDERS, *Emotions and Bodily Changes*. New York: Columbia University Press, 1935. Pp. xv+595.

"The present monograph represents an attempt to bring together in some sort of perspective the research material which has combined to develop those concepts in medicine which are the necessary foundation for further research in the sphere of psychosomatic problems." The reason for such an undertaking is that: "We lack perspective concerning our knowledge . . . and are confused in our concepts of the interrelationship of psychic, including emotional and somatic processes in health and disease. . . . Here as so often happens we know more than we know we know; in other words, the actual scientific information available, having been achieved along the lines of the several specialties, has never been gathered together, correlated and evaluated. . . . As a result the young scientist, interested in these problems, finds himself disoriented in the field of research he is choosing." And with the need thus expressed the author immediately and quite properly concedes the extreme difficulty of her task.

The near futility of an effort to bring together in a single volume knowledge from all pertinent fields so as to make a profitable synthesis must appear even before seeing the content of the book. Any attempt to survey the universe of psychosomatic relations in short space must of necessity be confined either to the collection of a

very few significant fragments from each field, or else be devoted to presentation of superficial snapshots of surface phenomena. A short *Outline of Psychophysiology* like an *Outline of History* obviously may be of value to the uninitiated, but it cannot supply a sufficient orientation to one seriously entertaining the idea of research. Whether we like it or not, advancement in the interrelation of scientific fields takes place through the correlation of highly specific phenomena which are of necessity the province of the specialist and cannot be conveyed by general presentation. It must be said that the author, especially toward the end of the book, recognizes this limitation and that she should not be too seriously indicted for her initial enthusiastic rationalization of her undertaking.

Without doubt the perusal of this compilation will suggest new ideas and new angles of approach even to one already conversant with the several fields. The bibliography of 2,251 titles occupying 125 pages is of real value, notwithstanding serious omissions and the fact that most of the material is not later than 1932. The material is presented largely in the form of excerpts from reviews of various studies. Sometimes one wonders why a given citation or review has been presented, apparently without regard to its intrinsic value. Sometimes the citations lead one to suspect a mechanical, non-critical assembly of adverse points of view. Important or outstanding contributions on various topics are sometimes glaringly absent. One is, for example, surprised that in a lengthy discussion of "horme" or purpose as a factor determining development and behavior there is no mention of McDougall's contributions championing the doctrine. Again we find three pages devoted to Marston's homily on materialism *vs.* vitalism and discussion of "psychonic energy," and immediately thereafter only a page and a half providing the orientation for Gestalt psychology. In that brief space, rightly emphasizing the probable relation of psychological to physiological Gestalten, Wertheimer's pertinent papers are not mentioned.

The author appears more thoroughly at home as the discussion leads to possible applications of psychoanalysis. Here the presentation, which is generally devoid of critical attitude, is that of the convert, although not that of the protagonist. Studies of ants, not too critically interpreted by Brun, contribute to the support of psychoanalytic theory. Morphological studies indicating psychosomatic relationships are also given appropriate consideration, although with no mention, even in the bibliography, of such important papers as

those of Von der Horst, Wertheimer and Hesketh, Sheldon, Shaw, and Mohr and Gundlach.

The problems of acute and chronic illness properly receive extensive consideration. The neuroses are discussed from many angles. Pavlov is quoted to the effect that we cannot interpret neuroses on the basis of any one recorded vegetative change, but that if we could study all the vegetative reactions at the same time we would find a picture which would be characteristic. The writer might well have mentioned the additional importance of correlating these vegetative patterns with concomitant patterns of the postural tensions. The relation of disease to emotion is pointed out with illustrations from Stratton and from Crile. Mühl shows how fear and hate may tie up large amounts of energy ordinarily available for maintenance of general bodily resistance. Deutsch identifies conversion symptoms with discharge of dammed up libido: "In the healthy body the libido circulates as unnoticed as metabolic products." Health is conditioned by absence of "bound organ libido." There is no effort to point out to the young investigator the need for establishing the neurophysiological mechanisms concerned. It might well have been emphasized that as we gain knowledge concerning the neurophysiological mechanisms we shall have less need for such hypostasizations as "bound energy" and "organ libido."

Perhaps the most inadequate portion of the book is that dealing with the problems of physiological measurement. The fact that physiological reactions attain psychological significance primarily as they provide symptoms of varied reaction to different aspects of the stimulating situation is not adequately emphasized. One might even gather that we should look for something psychologically significant in the physiological reactions as such. Ten pages are devoted to the galvanic skin reflex based largely on Landis' and Landis and DeWick's reviews. The questionable character of much of the work in this field is duly stressed. The observation that statements by one investigator are in practically all instances contradicted by negative results of some other investigator, whether or not under different conditions or with different techniques, reminds the reviewer of the negro, accused by six witnesses of chicken stealing, who endeavored to establish his innocence by the defense, "Judge, ah has six witnesses who didn't see me steal them chickens." The author appropriately calls attention to the generally observed decrease in the galvanic reactivity characterizing psychopathological conditions. She also points out that there may be a certain validity to studies of the rela-



tion of galvanic responses to ratings of the "emotional value" of stimuli.

Capillary microscopy is disposed of in ten lines. Extensive studies by plethysmographic methods are unmentioned. The verbal-motor reaction method of Luria is given a page of general discussion, but the prime essential to understanding of the method, a clear, brief description of the specific procedure, is lacking. The elektro-enkephalogramm is discussed in two pages in the light of Berger's original papers. "Biochemical methods, including laboratory techniques, thermometers, etc.," are disposed of in eight pages, seven of which are devoted to a helpful table giving some of the more outstanding differences observed under the various diagnostic classifications of mental disease. Three pages are devoted to Boas' cardiograph, with no mention of other instruments or of other pulse studies, for example that of M. Day. Compensatory effects upon heart rate attending increase in blood-pressure which so largely vitiate the value of all short time changes in heart rate are unmentioned. It is, however, observed that the cardiograph would be considerably more useful were it possible to employ it in conjunction with a satisfactory apparatus for continuous blood-pressure recording. The X-ray as a means of cardiac and gastro-intestinal observation is briefly considered, but no mention is made of the less hazardous balloon method of stomach, duodenal, and colonic investigation used by Cannon and Washburn, Carlson, and Brunswick. Blood-pressure as a measure receives consideration in a later chapter on the cardiovascular system.

In the conclusion of this chapter the author aptly points out the confusion which reigns in the case of practically all attempts to correlate single or even several random quantitative measurements, especially where the attempt is made to correlate such measurements with diagnostic labels rather than with specific mental states. The author further shows her appreciation of the problem by her suggestion that it is not alone the level of activity or even the amount of change in activity revealed by these measures, but data on the rate of change or the acceleration of the bodily processes under varying environmental demands, which is likely to prove important. Cannon's concept of "homeostasis" is appropriately considered in its bearing on the problem.

Part II of the book reviews certain more complex problems of psychosomatic relationship under the general heading of "Organs and Organ-Systems." First consideration is appropriately given to

the nervous system (only 10 pp.) with first emphasis upon the importance of considering the "organism as a whole." Stress is laid upon the effects of structure on function and of function on structure, and brief discussion is given of neuropsychiatric problems upon which whole books have been written. This is followed by a good chapter on "Musculature" with emphasis upon the rôle of muscular tension and tonus in mental function. A chapter of 29 pages on the "Endocrines" presents a bird's eye view of this extremely complex and rapidly advancing field of knowledge. The portion dealing with the specific glands is, however, necessarily both inadequate and out of date. The wonder of the pituitary, for example, is appropriately extolled, but the available knowledge even of 1933 is not too clearly or concisely set forth. Doubtless if the citations briefly referred to were studied, the picture would be more complete. Yet Cushing's work since 1913, for example, is not mentioned. In the case of the adrenals the medulla alone is discussed and the probably equally important influence of the adrenal cortex on mental condition is not referred to. In general it must be stated of this chapter that it is inadequate for one wishing to gain a general understanding of glandular functions, and it is useless for one who already knows anything about the problem.

A chapter on "General Metabolism and Heat Regulation" in its relation to psychic functions occupies 38 pages, special emphasis being given to acid-base equilibrium, sugar metabolism, and blood cell changes. Basal metabolism is more inadequately treated. There is, for example, no mention of Totten's study nor of Benedict's work since 1909, nor of studies by various workers using the method of total insensible weight loss. The chapter, nevertheless, contains much valuable material.

The chapter on the cardiovascular system brings together evidence especially of a clinical character, relating circulatory disease and blood-pressure changes to psychic states, and is 30 pages well spent. The succeeding chapter of 31 pages on respiration also contains much valuable material. Special emphasis is given psychogenic disturbances of the respiratory system, including asthma. The chapter on the gastrointestinal system presents at length (48 pp.) interesting and valuable experimental and case history material demonstrating psychic effects upon saliva, gastric secretion, bile, intestinal motility, etc. The chapter which follows on "The Genitourinary System" is likewise a valuable and on the whole well chosen collection largely of case history material treating more important psychogenic dis-

turbances. A chapter on "Special Sense Organs Exclusive of Skin" treats largely of factors disturbing vision which are causes or excuses for other bodily symptoms.

The chapter on skin is a somewhat detailed and extensive collection of references with the avowed end of demonstrating the important rôle of psychogenic factors in the production of skin disorders. The significance of skin sensations, tickling, and itching, as in many respects identical with effects of sexual excitation is extensively supported. Emotional precipitations of eruptions and other epidermal symptoms are cited at sufficient length to establish emotion as a frequent etiological factor. The suggestions contained in certain papers relating to the possible neurophysiological mechanisms involved are well chosen. The possibility that effects are precipitated by locally produced angioneuroses, that the effect may be due to autonomically induced secretion of histamine, that emotion may produce the same general effects in patients as the actual injection of foreign protein, and the suggestion of Stokes that cutaneous effects may be secondary to psychic disturbances of the gastrointestinal tract merit special mention.

Part III of the book is a brief chapter on "Therapeutic Considerations."

From the standpoint of the psychologist this book presents valuable collections of experimental and clinical material indicating the devious and too often inadequately emphasized pathways of psychosomatic influence. The book will be a valuable addition to the individual or departmental library notwithstanding the many important omissions which may not improperly be attributed to the difficulty of covering so wide a field within the scope of a single volume.

CHESTER W. DARROW.

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JACOBSON, EDMUND, *You Must Relax*. New York: McGraw-Hill Book Company, 1934. Pp. xv+201.

The subtitle reads, "A practical method of reducing the strains of modern living." The author notices that many Americans find it hard to meet the demands of their occupations, families and associates, and therefore keep much of their musculature under high tension during much of the time. He believes that on the average, their muscular tension is higher than that of their predecessors (who need fear only Indians, crop-failures and famine, wars, pestilences, and possibly the hereafter). This belief does not rest on measurement

and comparison, for only a few members of the present generation have been studied; and none of the past. He assumes that unnecessary muscular tension implies unnecessary nervous activity; and he believes that if the latter is persistent it is also bad. Why it is bad, he does not say. While he speaks now and then as if activity damages nervous tissue, he also mentions (p. 128) that no direct evidence of damage has been found; and no evidence that "wasted" nervous activity (such as may be manifested in fidgeting, frowning, and stiff poses) indicates that bodily energy is being used up faster than it is being supplied, or that waste-products are being manufactured faster than they are disposed of. Occasionally, he hints at a more plausible hypothesis: namely, that it is the *distribution* of nervous activity and of muscular tensions that counts. In plain words, if a person *tends* to perform one set of actions while he is executing another set that is incompatible with the first, then he behaves clumsily, ineffectively; he substitutes worry for genuine thinking; he also behaves emotionally. All this implies tension—a tendency to activity in those parts of the neuromuscular apparatus that are not required for effective response to the actual situation. The author believes that unnecessary and persistent muscular tension makes for insomnia, habits of worry, habitual fears, spastic intestines, and the like; he also suspects that it at least contributes to the syndrome of high blood-pressure.

He suggests a two-fold remedy: (1) "differential" relaxation, *i.e.* the non-use of muscles which do not contribute to the activity-pattern which the person has chosen to execute; and (2) "general" relaxation if the external situation demands or permits no specific responses to it. Three chapters of the book are devoted to instruction in the art of achieving these two kinds of relaxation, beginning with the detection of the state of some particular muscles through introspective observation. Another chapter briefly indicates how the physician can help the patient acquire this art, the complete directions being given in the author's earlier book, *Progressive Relaxation*.

One should distinguish (a) a certain therapeutic method from (b) the author's rationalization of it. (a) As to the method itself, the reviewer, being favorably impressed with many plausible reports of its results, has advised several nervous and insomniac individuals to consult the author or some physician whom the author recommends; and in at least some of these instances, the procedure appears to have been helpful. (b) But to "justify" the clinical procedure (which

ought to be judged by its results) the author presents a theory that rests on certain very doubtful presuppositions.

(1) It is presupposed that voluntary relaxation of a muscle is purely negative, in the sense that it implies a *lack* of nervous activity. But we may at least *entertain* the hypothesis that relaxation implies a *transference* of nervous activity from those fibers which excite the contractile organs to those fibers which excite the posture-holding organs. By the second hypothesis, relaxation of a muscle is a positive response to stimulation; even when one is well relaxed, his sense-organs and some parts of his central nervous system continue to act, only the energy is discharged into other effector-organs than those which produce movement. A test of the two hypotheses can be conceived; perhaps eventually it can be performed. Meanwhile it may be better not to try to settle the matter by presupposition.

(2) It is presupposed that sleep results from removal of sensory stimulation (pp. 172 ff.). This doctrine is fashionable, but it contradicts certain facts of observation. Protection from light, for example, is not necessary, nor is it sufficient, to induce sleep. Many animals sleep in the full glare of the noon-day sun; and many human patients learn quickly to sleep under the same conditions—in sanatoria, for example. It is questionable whether the metabolic activity of the retina does not excite the dark-adapted retina as highly as light excites it under ordinary conditions. Again, when a person lies down, he removes one set of stimulus-patterns, resulting from tension in the muscles that serve to maintain an upright pose. However, he probably substitutes for it a new pattern of tensions in the muscles that maintain his recumbent posture; he also introduces a new pattern of pressures on skin and other soft tissues, of stresses in tendons, of pressures in joints, a new pattern of thermal stimulation in the part of the skin that touches bed-coverings and mattress, a new pattern of fluid-pressures in the viscera, etc.

(3) It is presupposed that when a person is sleeping deeply, most of the muscles in his limbs and trunk are well relaxed. If this is true, then I fear that deep sleep is a rarity. I have examined the photographic records of some 20,000 poses taken by healthy sleepers. Perhaps once or twice in a whole night, a young child will assume a pose in which he can remain limp, but he seldom assumes such poses, and adults almost never. To test this presupposition one must first settle the definition of a few terms. What does one mean by the "deepness" of sleep? Of *what events* is the sleeper "unconscious" and of *what events* is he "conscious" at any given instant? What



is the criterion of "consciousness" of those events: quiescence of a detector of tension in some single muscle, or some criterion independent of the behavior of this detector? In the former instance, the presupposition means that the detector detects what it detects, which is a truism; in the latter instance, the presupposition implies that some independent variables can be exhibited, and correlated with the readings of this detector. This appears not to have been done; in fact the so-called "depth" of *general* sleep cannot be detected and measured, one reason being that if an organism is alive it cannot behave so as to satisfy the usual definition of "general" sleep.

(4) It is presupposed that all sensing, perceiving, imagining, thinking or willing is accompanied by a pattern of movement or at least of tension in the skeletal muscles that corresponds to the kind and quantity of mental activity. This is the doctrine of Descartes. Many modern psychologists *assume* it to be true; this is an act of faith. Not only is the evidence inadequate; the hypothesis has not yet been formulated so as to permit one to conceive a genuine test. For example: (a) Is the correspondence between movement and mentation of the one-one variety? If so what is its *form*? Give us the rule, and then *if* we can detect the activity-pattern in the musculature we can infer the mental event. Is it *reversible*, so that given the mental event, the subject can infer the corresponding muscular activity-pattern? (b) Is the correspondence not one-one, but *one-many*; or is it (c) *many-one*; or is it (d) *many-many*? Still worse, is it (e) *incomplete*, so that some mental events are not registered in tension-patterns, or some tension-patterns not accompanied by mental events, or both?

These questions are not settled satisfactorily by presupposition, for all that the latter permits is a re-naming of what one observes. But neither can they be settled experimentally by any means that are known at present.

In favor of this hypothesis the author mentions certain findings which are actually irrelevant to it. He asserts (p. 172) that when his sleeping subjects were connected to a detector of the action-currents of some muscle, some of them, after an interval of rest, would excite the detector. "If at such an instant the subject is awakened by the operator, he reports having been dreaming. Our findings, then, suggest . . . that the deepest sleep (the most relaxed sleep) is free or relatively free from dreaming." But this procedure does not show *when* the sleeper dreamt, *i.e.* before he tensed the muscle, while he was tensing it, or afterwards—especially when the

operator aroused him. Nor does the author mention a necessary control test, namely, arousing the subject while he is lying relaxed, and asking him about his dreams.

To the reviewer it seems that a clinical practice which can be judged on its merits ought not to be represented as resting on so doubtful a rational basis as this. Certainly if the latter should be destroyed, the practice would continue as long as the physician and patient were convinced that it was useful. The theory therefore seems to the reviewer to be a luxury.

For the most part the book is well written. There is no index.

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RUSU, LIVIU, *Essai sur la création artistique: contribution a une esthétique dynamique*. Paris: Félix Alcan, 1935. Pp. 460.

No problem deserves more attention from the psychologically minded aesthetician than that of artistic creation. No field has a richer theoretical or biographical literature. And yet, owing to the essential complexity of the subject and to the discursive nature of the literature, few have had courage to make a direct attack upon the problem. This Dr. Rusu has done with admirable clarity.

His book is divided into four parts. The first deals with the origin of the creative impulse, the second with the preparatory or, as he calls it, *unconscious* phase of the creative process; the third, with the conscious phases—inspiration, elaboration, and execution; and the fourth with the different types of creation and their application to aesthetics.

In part one the origin of the creative drive is traced to disequilibrium produced in the organism by unsatisfied organic needs. Artistic expression, he believes, is always in terms of *movement* reflecting these inner tensions, and is aimed at a restoration of balance within the self or with the environment. Creation is not accidental, as the behaviorists have always maintained, but is forced upon the artist by the necessity of resolving such inner conflict. The artist, in short, cannot live without creating.

Dr. Rusu rejects the origin of art as lying in the practical aims of everyday primitive life, as he rejects also its specifically sexual origin. Art is not a discharge of simple impulses, *i.e.* a kind of mental purgation. Nor, again, is it play. Play manifests less severe maladjustments, and is itself sufficient to overcome them. Art has as its basis

a conflict which involves the most profound psychic depths. For the resolution of this conflict creation (integration) is alone sufficient.

The second part of the study deals with the preparatory phase of artistic creation. The artist, having wide previous contact with environmental reality, reaches back into his past for the material out of which his new conceptions are formulated. At this stage the creative integrations are still latent. Later, when they appear in consciousness, they constitute an *insight*, or *inspiration*, which must, of course, be brought to deliberate formulation and elaboration. Naturally, this exteriorization requires great effort; inspiration becomes an act of will. Yet the integration which it evidences is not itself art, but becomes such only when it can be captured and redressed.

Before objectification can take place, however, the conflict produces a period of mental chaos or tension tending to disintegrate any previously established unity of mind. The creator feels keenly the need of introducing an order into his formless impulsions that he may appease his restlessness. The inevitable pattern which the work finally assumes has here its origin. And the freedom which the creator longs for has also this same origin—a peace which is a resultant of psychic forces, not an exhaustion or negation of them. Pathology results when the artist cannot find such balance. Thus in the preparatory phase the invisible sources of creation little by little become conscious, and are finally through struggle made concrete in expression in the form of the work of art.

The third part of the book deals with the conscious phases of the creative act—inspiration, elaboration, execution—phases merging into each other by imperceptible degrees. Inspiration, *insight*, appears suddenly and acts as a bridge, or interconnection, between the unconscious and the conscious. In the measure that insight is an integration of both these phases of mental life, it restores equilibrium and peace. The degree of release, exaltation, etc. discovered in the moment of insight is proportional to the degree of inhibitory tension hitherto engendered by the conflict. Once arrived in consciousness, the insight may have various degrees of clarity. Dr. Rusu deals further with such aspects of the creative act as the central moment of inspiration, the dynamics of inspiration, its types, the conscious labor of elaboration, creation and will, material and form, the types of execution, etc.

The implications of these theories for aesthetics constitute the final chapter of the work. The problem of the beautiful becomes

envisaged under new terms. A work of art pleases us not because it expresses beauty in itself, but because it incarnates a special revelation of the world. Displeasure comes not because the work expresses the ugly, but because it expresses nothing spiritual at all. All depends upon the degree of objectification of inner reality which expression can give. Other aesthetic categories also must be considered under new aspects—the comic and the tragic, etc. depending upon the particular drainage which insight gives to the unconscious.

Although I have some question as to whether the preparatory phase of artistic creation is entirely unconscious, as Dr. Rusu maintains, I find myself in large agreement with the general point of view of this book. Its chief contribution lies in its theoretical organization of the field of creative thinking rather than in the originality of its illustrative materials. The style is clear, simple, adequate. A bibliography and table of materials are an aid to further research. Without question, a distinct contribution to the literature on creation!

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CONKLIN, EDMUND S., *Principles of Abnormal Psychology*. (Revised.) New York: Henry Holt and Co., 1935. Pp. xiv+527.

Numerous problems concerning the subject-matter and mode of presentation confront the scientist who would expound this difficult field. The favorable reception accorded this author's first edition of 1927 warrants comparison of his present plan with that of the edition of eight years ago. The revised edition is increased in size about 70 pages and also reveals certain changes in point of view, arrangement, and choice of subject-matter.

Writing as a psychologist primarily for advanced undergraduate students in psychology, the author in both editions gives the impression of being at great pains to steer clear of issues which might conceivably concern the clinical art of treating persons manifesting abnormalities of behavior. The latter is to be the task of psychiatry and the psychiatrist; whereas psychology, we are told, should stand apart as a basic science.

This point of view adopted in a text necessarily colors the presentation and pedagogy. Whether such a distinction is intrinsically sound, or a passing reflection of current professional sensitivities which may presently give place to a more fruitful spirit of team-play between psychology and medicine, such as physiology and other basic

sciences enjoy with clinical practice, only the future of psychological medicine will reveal. To foster this familiar distinction of purism at least misses two points that today are of primary importance to psychology and psychiatry alike: (1) that an increasing number of leading medical schools are seriously endeavoring to introduce into their curricula an adequate basic psychology such as will be helpful to all practitioners and not simply to psychiatric specialists, and (2) that consequently an increasing number of psychologists will be faced with the problem of teaching their subject to students of medicine as well as to other college students. The task of bridging the lamentable gap now existing between 'pure' psychology and its applications towards health, *i.e.* towards the preserving of mental health as well as the combating of ill health, cannot wisely be left wholly to the student; the teacher and the text should share this responsibility and particularly texts that concern abnormal psychology.

Happily our author in his revision has made a start in this direction by including a new chapter on psychotherapy, although he states in the preface that there is some question on his part of the propriety of so doing. Somewhat apologetically he continues in justification of this innovation in these terms: "Nevertheless, there is no reason why the educated layman should not know the basic principles of current psychotherapeutic methods. . . . It is even possible that the dissemination of such information might contribute not a little to a better popular attitude towards psychiatry. . . ." May one hope that the next revision will venture to be even more courageous and constructive, in order at least to suggest to the profession as well as to the laity that there exists a problem of prevention as well as of treatment in connection with the field of the abnormal. This, indeed, will give an added touch of reality and meaning to the subject for every educated person, perhaps more so than will mere didactic information about technical therapeutic procedures.

In harmony with his decision to make reference to principles of psychotherapy, other textual changes follow in train. First, he gives a more systematic presentation of psychogenesis of several of the forms of abnormality. This is now stressed more positively than in the previous edition and moved from the middle to the opening chapters of the work. Second, the main pedagogical framework of the text is likewise modified. Instead of proceeding from introductory terms and interpretative concepts directly to extreme forms of abnormality and thence back to borderline phenomena, now we are taken progressively from the introductory and psychogenic concepts.



through derangements of simpler functions (sense and memory), into milder personality disorders, thence to abnormalities of complex functions (speech and thought), and only then to major forms of abnormality, in brief, an arrangement which approximates in some measure a hierarchical psychological series. In the reviewer's opinion this is unquestionably an improvement in plan, even though it does not embrace all of the topics that are treated—always a baffling task in classification for writers of psychological texts, whether of the normal or the abnormal.

The order and scope of subject-matter in the 21 chapters (three of which are new) may be briefly summarized:

<i>Subject-Matter</i>	<i>No. of Chapters</i>	<i>Approximate % of Page Space</i>
Preliminary.....	2	10
Dysfunction of Simpler Processes (Sensation, Memory)	2	9
Milder Disunities in Personality.....	3	13
Complex Mental Dysfunctions (Speech, Thinking) ..	2	6
Major Forms of Abnormality (Psychoses).....	3	10
Abnormal Endowment (Subnormal, Genius).....	1	7
States of Hypoactive Behavior (Sleep, Encephalitis)	2	8
Special Mental States (Hypnosis, Dreams).....	2	15
Psychotherapy.....	1	5
Drugs.....	1	5
Spiritistic Phenomena.....	1	8
Historical Backgrounds.....	1	4
Index (Author and Subject).....		13

This is substantially similar content to that in the first edition, though with expansion of some topics and four principal additions. The latter include: (1) a good though brief chapter of 13 pages on speech phenomena, (2) another of six pages on behavior in epidemic encephalitis, (3) a brief reference to the condition professionally known as constitutional psychopathic inferiority, and (4) the new chapter of 26 pages on psychotherapy. Of these the first and last will probably be regarded the most significant.

No doubt the author has considered other possible additions but refrained. The above selection was added mainly, he says, on the behest of friends and critics. On the other hand what principle should govern selection? A tentative definition of the field (p. 2) is that abnormal psychology may be thought of as the study of those forms of human behavior which differ sufficiently from those which are commonly accepted as normal to be recognized as different, irregular, or disordered (cf. p. 178). This may provoke questions of systematic import. The determination of abnormal behavior is to be made by methods of observation, objective or subjective or both.

Also, etiologically, abnormal forms of behavior, we are told, may derive organically, or functionally, through conflicts with the social order, or through development of conflicting patterns of organization within the personality—by combinations of circumstances within and without.

In order to test these principles, is, then, delinquency admissible as a form of abnormal behavior? Evidently not, at least it is not elaborated as such in the text or index. Is this because in the last analysis abnormality for psychology is to mean only conflict recognized as such from within, *i.e.* by the reacting individual? If this be postulated, we still are scarcely out of the woods about what matter to select, since the interpretation of behavior involves evaluation and recognition of adjustment and maladjustment of persons (p. 8). And whose judgment is to count, that of the reactor or an outside evaluator? If we do not here misread the author, apparently the former is to be the final court of appeal for a basic psychology of abnormal behavior. "The well-adjusted person is one whose patterns of reaction are harmonious and suitable to the situations of life as they are perceived. Of course there may be large differences in the way the situations of life are perceived by different individuals, and their dominant reactions may as a consequence be different; but, if to the world as perceived, the reactions are suitable and harmonious then that person may be thought of as adjusted. The maladjusted person must obviously be the converse. . . ."

From this systematic point of view what exactly are the criteria for the 'suitability' and 'harmoniousness' of reactions as so perceived by the reactor? The reviewer is unable from the text or by conjecture to answer convincingly. He suspects that the evaluation of behavior as poor adjustment or good, *i.e.* as abnormal or not, turns in part upon psychological judgment from within, namely, as immediately perceived by the party in question, and in part upon biological or sociological judgment as objectively practiced by others about the party so situated, especially by those in social control. But if these criteria of adjustment be accepted, might not delinquent behavior legitimately be included among forms of abnormal behavior? In any event it is a noteworthy segment of human behavior that has to be dealt with, whether or not abnormal psychology can contribute to its understanding and control.

Principles of psychotherapy are discussed in terms of three main steps: securing a life history, discovering the type of personality, and recovering repressed material by direct or indirect methods. Finally,

treatment of the personality may involve, we are told, not merely the unfolding of facts in these steps but an elaborate process of reeducation. This latter important portion of the picture, however, remains relatively inadequately sketched (p. 423). Unlike the usual care by the author to present for the reader every alternative scientific approach to the factual matters in question, with judicious comment on their respective merits and limitations however conflicting these views may be, one feels that he here leans heavily upon certain of the better known psychoanalytic views and procedures. There is no mention in this connection, for example, of the influential work and teaching of Adolph Meyer in terms of "psychobiology," which aims to be analytic, psychological and reeducative, without necessarily being psychoanalytic. Moreover, if the time should come when principles of prevention as well as of therapy might find mention in treatises on abnormal psychology, would not then the fundamentals of the normal learning process to which psychology has contributed so much, also appear in the picture of how behavior operates? In short, mental adjustment would mean always a process of education and sometimes of reeducation.

Again, to those who deal more expertly with abnormal psychology, may we fellow workers make one other friendly plea for help? Expressions pertaining to emotion, emotional experience, emotional reaction, emotional instability, untold kinds and conditions of emotion, constitute a persisting refrain through texts on abnormal psychology. Yet what about emotion, granting its basic importance? Has psychology yet a satisfactory scientific schema for dealing unconfusedly with whatever aspects of John Doe's existence these and such like terms actually connote? Or have we often in too large measure simply an elastic but undefinitive terminology? If it be accepted that basically our science will advance in proportion to the degree of clarity we can inject into its basic concepts, we shall perhaps be well advised not to take too much for granted in our students or readers concerning their critical understanding of even our commonest terms. The space introductorily given to leading concepts in the present work is therefore well justified.

Documentation is given with that meticulous care for detail and accuracy for which the author is well known. References to the literature are selected in terms of their worth and probable accessibility and are presented at the foot of the page with general bibliographies at the end of the chapters. Footnotes of the sort that so frequently heckle scientific texts are happily conspicuously lacking.

References in the present edition have been expanded to include the growing body of experimental studies relating to abnormal phenomena, which adds to the many values of this work another feature of importance, namely, its value as a scientific source book.

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SHAFFER, LAURANCE F., *The Psychology of Adjustment*. Boston: Houghton Mifflin Company, 1936. Pp. xix+600.

The appearance of Shaffer's book will be a source of great satisfaction to professional psychologists. It is the first attack on the problems of mental health and adjustment which has made use of the available resources of scientific psychology. Although by almost universal consent human adjustment to human circumstance and the failures of adjustment are matters of learning and habit, no previous writer has taken the trouble to base a treatment of mental health on a sound theory of learning. Shaffer has made a sound and conservative theory of learning the foundation of his treatment of the sources of maladjustment and of the methods available for cures.

Of the four parts of the book the first reviews the general psychology underlying the description of behavior as adjustment. It contains a very clear, very readable, and very well considered account of the origins of behavior, the ways in which behavior is modified, and the nature of drive and motives.

This is done substantially in terms of stimulus and response. The treatment of learning is in the associationistic tradition of Hobhouse, Lloyd Morgan, and E. B. Holt. The basic principle of learning is conditioning or association by contiguity. In general Holt's conception of the nature of conditioning is followed, with some modifications originating with Pavlov. The treatment of motivation is similar to Holt's or Dashiell's with the original sources of drive conceived as tissue conditions, stimuli, or muscular tension, and the more elaborate social motives derived from these through associative learning. Throughout the book the principle of conditioning is supplemented in the attack of problems of adjustment by a reformulated Law of Effect: animals tend to repeat and to learn those responses that lead to tension reduction and to the completion of motivated activity. Much good use is made of this descriptive generalization.

The second part of the book deals with the varieties of adjustable behavior, adjustments by defense, adjustments by withdrawing, the rôles of fear and repression in adjustment, adjustment by ailments,

and the forms of persistent non-adjustive reactions. In general, undesirable adjustments are explained as responses which have proved in disturbing situations to be tension-reducing and thus fixed as habits. Repression is described as failure of recall, acquired because it resulted in partial reduction of tension. Its unfortunate effects consist in the fact that the reduction of tension is only partial and in the fact that failure of recall eliminates the most available means of reëducation.

The third part of the book is devoted to personality and has chapters on the measurement of traits, the organic influences in personality, and the development of personality traits. There is included a very clear and critical account of psychoanalytic theory on the ground that students should be familiar with it and with its terminology.

The last four chapters of the book concern techniques of mental hygiene and are filled with definite and specific suggestions for the clinical psychologist and with excellent descriptions of good clinical practice.

The book is a splendid example of what can be done with scientific and objective methods in describing and explaining the forms of human maladjustment. From it are missing the demonological characters of the Freudian drama, the too-easy lists of instincts used by the psychologies of the recent past, and the pseudo-mathematical vectors and valences now becoming popular with some writers. Where social motives like mastery or security are used in explanation of conduct, their analysis in terms of stimuli and drives is provided for.

Minor faults in the book include an occasional uncritical acceptance of the Pavlovian notion of extinction. It is not true that "unsupported" associative cues regularly lose their effectiveness with repetition. This happens only when circumstances interfere with the response. Shaffer's practical insight leads him to recognize this when he explains catharsis in terms of extinction and then says: ". . . improvement is effected by recall of his experience in the non-fearful situation of the interview, thereby extinguishing the emotional response that provided the intensity of his motivation" (p. 483). This is not extinction but inhibitory conditioning.

Forgetting is summarily treated as an effect of the lapse of time or of Pavlovian extinction, which is very dubious theory and would imply that time heals all wounds, which is just what fails to occur in the neuroses. Tension-reduction is described as an empirical principle as yet unexplained. It may be suggested that when tension reduction does operate to fix habit it probably does so because the



removal of tension and of the maintaining stimuli responsible for the tension leaves the animal without any opportunity for reconditioning. The stimuli responsible for tension and the tension itself are not there to be reconditioned by intervening experience and forgetting cannot occur. The reviewer regrets also that there is no consideration of Janet's very important descriptions of cures by stimulation which would have been quite amenable to Shaffer's general method.

In the reviewer's opinion this is by far the most sound and the most useful book which has yet appeared in the field of mental hygiene.

E. R. GUTHRIE.

*University of Washington.*

COLLINS, MARY, and DREVER, JAMES, *Psychology and Practical Life*. London: University of London Press, 1936. Pp. viii+307.

According to the preface this book is intended to give the "intelligent reading public" a general account of applied psychology and also to serve as an elementary textbook for students. It fulfills both of these purposes with reasonable adequacy although with regard to the latter purpose it is almost too elementary. The discussion opens appropriately with a consideration of individual differences and describes some of the conventional tests. A chapter on childhood and adolescence stresses mental hygiene at these levels with some mention of sublimation and fixation. Then follows a discussion of the measurement of intelligence with a description of typical individual and group tests. Mention is made of social intelligence, tests of temperament, character, and association, and rating scales as measures of personality. The authors are a bit optimistic in stating that the association tests give "amazing insight" into personality. Some of the procedures described are likewise out of date, such as the Downey Will-Temperament Test. The chapter on learning gives the traditional laws and some suggestions for economy of memory.

The remainder of the book turns to the more psychotechnical problems, beginning with vocational guidance and selection. In the former the most stress is laid upon intelligence measurement and in the latter the treatment is rather cursory. The validation of tests is mentioned but no indication is given of such techniques as weighting the tests. The chapter on "Psychology of Work" mentions practically all of the problems in industrial psychology very briefly and gives a good superficial survey of the field. The discussion of advertising follows the outline originally propounded by Hollingworth.

In connection with health there is considerable stress on hypnosis as a therapeutic means and also some discussion of psychoanalysis. The last chapter on social problems considers the feeble-minded and especially their relation to delinquency, goes into the philosophical and psychological aspects of punishment, and ends with a mention of some sources of error in testimony.

The work is for the most part written down to the level of the layman and presupposes no other psychological background. However, the authors are not always consistent in this program and occasionally introduce something disproportionately technical. For example, Spearman's general factor is broached rather suddenly in an otherwise popular context and the correlation coefficient is mentioned as "an application of a well-known canon of inductive logic." Attention is called to certain contributions in the British literature that some of us may have overlooked. In the psychotechnic portions, the book affords a good outline of the field. This would be an entirely appropriate and conservative little volume to put into the hands of the layman interested in psychology. It would also be suitable for a cursory introductory course with no psychology prerequisite. The professional psychologists will find nothing in it that they do not already know.

HAROLD E. BURTT.

*Ohio State University.*

#### BOOKS RECEIVED

FILLMORE, E. A., *Iowa Tests for Young Children*. University of Iowa Studies, Studies in Child Welfare, Vol. XI, No. 4. Iowa City: University of Iowa, 1936.

GASKILL, H. V., *Personality*. New York: Prentice-Hall, Inc., 1936. Pp. vi+52.

LINK, H. C., *The Return to Religion*. New York: The Macmillan Company, 1936. Pp. 181.

TESCHITZ, KARL, *Religion, Kirche, Religionsstreit in Deutschland*. Kopenhagen: Sexpol-Verlag, Postbox 827, 1935.

YOUNG, P. T., *Motivation of Behavior*. New York: John Wiley and Sons, Inc., 1936. Pp. xviii+562.

## NOTES AND NEWS

DR. DAEL L. WOLFLE has resigned as professor of psychology at the University of Mississippi to accept a position as Examiner for the Division of the Biological Sciences at the University of Chicago.

DR. JOHN B. WOLFE has accepted an appointment as professor of psychology at the University of Mississippi beginning September 1, 1936.

AN anonymous donor has given £10,000 for the establishment of an Institute of Experimental Psychology at Oxford University. A decree has been proposed allocating to the Institute £500 and £150 a year for five years from the fund of the Rockefeller Foundation for research in social sciences.—From *Science*.

PROFESSOR CLARK L. HULL of Yale University was elected a member of the National Academy of Sciences at the meeting in Washington on April 27-29.

DR. STEUART H. BRITT, of the division of psychology, Institute of Educational Research at Teachers College, Columbia University, has been appointed to an assistant professorship of psychology at the George Washington University, Washington, D. C.—From *Science*.

AT its meeting in April, 1936, the Committee on Grants-in-Aid of the National Research Council made the following grants in the field of psychology.

Leonard Carmichael, professor of psychology, Brown University, "the onset and development of visually controlled behavior in fetal and newborn mammals"; Paul E. Fields, professor of psychology, Maryville College, "intercorrelation of sensory discrimination abilities in the white rat"; Ernest R. Hilgard, assistant professor in psychology, Stanford University, "quantitative characteristics of the process of acquisition and extinction of conditioned responses in man"; William A. Hunt, assistant professor of psychology, Connecticut College, "behavioral response to a shot stimulus"; Theodore Karwoski, assistant professor of psychology, Dartmouth College, and Mason N. Crook, associate professor of psychology, Skidmore College,

"study of the peripheral retina"; Ernest G. Wever, associate professor of psychology, and Charles W. Bray, assistant professor of psychology, Princeton University, "determination of the distortion of the ear as shown in the responses of the cochlea and auditory nerve"; Lester E. Wiley, assistant professor of psychology, Ohio Wesleyan University, "empirical testing of the theoretical learning curve derived by L. L. Thurstone."

Since the funds which have been placed at the disposal of the National Research Council during the past few years for the making of research grants have been discontinued, there will be no further meetings of the Committee on Grants-in-Aid.

THE Social Science Research Council has announced the appointment of twelve Post-Doctoral Research Training Fellows and ten Pre-Doctoral Field Fellows with stipends totaling \$65,000, and the award of forty grants-in-aid of research, totaling \$23,650, for 1936-1937. Among those who will receive grants-in-aid is Richard L. Schanck, instructor in psychology, Harvard University, for a study of the comparison of an English and a New York rural community from the standpoint of the behavior of the individual. Among the Post-Doctoral Research Training Fellows is Wayne Dennis, assistant professor of psychology, University of Virginia, who will study cultural anthropology as a background for experimentation in child development, at Yale University.

The closing date for applications for Post-Doctoral Research Training Fellowships and Pre-Doctoral Field Fellowships has been changed from December 1, 1936, to February 1, 1937, and the age limit for the Pre-Doctoral Field Fellowships has been changed from 27 to 29.

A CONFERENCE, representing one of the first attempts in this country to bring together leaders of two widely different schools of psychological thought with a view to developing methods of coöperative research, was held Saturday and Sunday, April 11 and 12, 1936, at the Menninger Clinic in Topeka, Kansas. A group of psychologists, including Dr. Kurt Lewin, author of the recent book, *The Dynamic Theory of Personality*, and a leader in the movement to develop a topological psychology, met with several psychoanalysts and psychiatrists of Topeka and Chicago to discuss the relationships between experimental psychology and psychoanalysis. The following program was presented:

APRIL 11, 10:00-12:30 A.M.

J. F. BROWN, Kansas University and Menninger Clinic, *Chairman*

1. *The Place of Psychoanalysis in the History of Science.* R. H. Wheeler, Kansas University.
2. *The Constitutional Approach.* H. Shuey, Boys Industrial School of Kansas.
3. *Relationships Between Topological and Psychoanalytical Psychology:*  
A. *General Relationships.* K. Lewin, Iowa University. B. *Topological Concepts Applied to a Psychiatric Problem.* R. G. Barker, Iowa University.

APRIL 11, 1:30-5:00 P.M.

KARL MENNINGER, Menninger Clinic, *Chairman*

1. *A Theory of Continuity in Dreams.* Thomas M. French, Chicago Institute for Psychoanalysis.
2. *Methodological Remarks on Both Psychoanalysis and Experimental Psychology.* H. FEIGL, Iowa University.
3. *General Round Table Discussion of the Possibilities of Coöperative Research.*

APRIL 12, 10:00-12:00 A.M.

KARL MENNINGER, Menninger Clinic, *Chairman*

1. *The Psychiatric Physical Examination.* N. Reider, Menninger Clinic.
2. *Preliminary Report on Experimental Studies in Psycho-Pathology.* J. F. Brown, Kansas University.
3. *A New Test of Conception.* J. Kananin, Chicago Institute for Psychoanalysis.

THE Institute for the Study of Animal Behaviour has been formed by a number of zoölogists, naturalists, physiologists, and psychologists with the object of promoting and encouraging research into animal behavior. Julian S. Huxley has been elected president for 1936-1937. The Institute intends to act as a clearing-house for information regarding work that is being done upon animal behavior in all its aspects, and to bring together for the discussion of their problems field and laboratory workers. It is hoped to issue to members a quarterly bulletin in which will be provided summaries of and references to the chief work being done in the various branches of the subject. Meetings for the transaction of scientific business will be held at intervals.

The Institute hopes eventually to obtain the means of supporting research and, possibly, of maintaining its own research station.

Those whose work is concerned with animal behavior will materially assist the task of the Institute if they will send offprints of their papers to the Secretary, R. C. Oldfield, The Psychological Laboratory, Cambridge, England.



AN oil portrait of Dr. Raymond Dodge, professor of psychology at Yale University since 1924, has been presented to Yale University by friends and colleagues of Dr. Dodge. The portrait, the work of Lloyd Bowers Embry, of the Yale School of the Fine Arts, has been hung in the Institute of Human Relations.

THE Eleventh International Congress of Psychology, which was to be held at Madrid, September 6-12, has been postponed until the last week of July, 1937.

#### PUBLICATION LAG IN THE PUBLICATIONS OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.

Actions of the Round Table on the Time Lag in the Publication of Psychological Research was reported by the Chairman, John E. Anderson<sup>1</sup>. This problem was discussed at a recent meeting of the Board of Editors of the Publications of the American Psychological Association, Inc. It has been a problem facing the Editors for a number of years. Reports made by each of the several Editors indicate that the degree of lag is not as great as was indicated at the Round Table so far as this group of journals is concerned, at least. The reports are as follows:

*Psychological Abstracts* (Walter S. Hunter, Editor): "No real lag—delays being in the abstractor's work."

*Psychological Bulletin* (John A. McGeoch, Editor): "A median lag of under six months—shorter for material such as reports of society meetings, special numbers, etc., contracted for in advance—longer than six months for other material."

*Psychological Review* (Herbert S. Langfeld, Editor): "Lag of no more than six months."

*Psychological Monographs* (John F. Dashiell, Editor): "Lag of over a year. This is especially long at the moment due to the acceptance of a commemorative volume of some 30 papers, as well as a set of 28 monographs from one laboratory—which represents an unusual condition."

*Journal of Abnormal and Social Psychology* (Henry T. Moore, Editor): "Lag of approximately 18 months. Occasionally it is possible to crowd in a paper of less than 10 pages before this time depending on the way the pages work out for any particular issue."

<sup>1</sup> *Psychol. Bull.*, 1935, 32, 855-858.

*Journal of Experimental Psychology* (Samuel W. Fernberger, Editor): "Present lag of 11 months which has been reduced from a lag of 18 months effected by radical selection policy."

Several matters must be considered in the question of time lag in publication. (1) It must be remembered that it takes an average time of six weeks for a manuscript to go through the press. This includes time for editing, composition, sending galley proof to the author and receiving his corrected proof, making up the number, correction by the editor of page proof, printing, binding, etc. (2) It is the belief of the Editorial Board that a lag of less than six months is probably not desirable inasmuch as this might hinder a policy of strict selection of articles—which policy will eventually raise the level of the journals. (3) It is the belief of the Editorial Board that the lag can best be reduced by strict selection and strict editing of the papers accepted for publication rather than by any other means suggested at the present time.

On this basis, it will be observed that only the *Journal of Abnormal and Social Psychology* and the *Journal of Experimental Psychology* have a lag seriously beyond the ideal.

It will be understood that the above is the opinion of the Editors of the Publications of the American Psychological Association, Inc., only, and in no way intends to imply the attitude of Editors of journals outside of this group.

May 1, 1936.

